Halle Studies in the Anthropology of Eurasia

Max Planck Institute for Social Anthropology

Studying Peoples in the People's Democracies II

Socialist Era Anthropology in South-East Europe

Edited by Vintilă Mihăilescu, Ilia Iliev and Slobodan Naumović

LIT

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This volume is a follow up to volume 8 of this series, which explored socialist era anthropology in East-Central Europe. On this occasion the countries investigated are Romania, Bulgaria and Serbia. In all three the discipline (irrespective of its local fragmentation) originated and developed as a 'nation-building science'. Scholars drew on the model of German *Volkskunde* and there was little or no interest in comparative *Völkerkunde* researches.

The contributors to this volume outline how this intellectual endeavor was affected by formally internationalist but in substance deeply national versions of socialism. Anthropologists were able by and large to nurture and sustain their special relationship to the nation under dramatically altered conditions, reacting more or less skillfully to fluctuating political pressures and eventually finding a secure niche for themselves in 'national communism'. Even though it was not instrumentalized in the same way by politicians and cultural officials, this national communism was found throughout the region. Anthropology was reinvigorated and institutionally consolidated as a science deeply concerned with national issues. There was considerable investment in 'monumental' projects, notably the expansion of ethnographic holdings in museums and the compilation of national atlases. Inevitably the ideological constraints set by the Communist Party led to theoretical and methodological distortions, but some creative scholars were able to develop individual strategies to overcome or at least evade them.

In order to provide a broad coverage of highly differentiated local landscapes, the editors invited contributions from scholars of different generations and institutional backgrounds, including for each country a distinguished American anthropologist to present a view from outside. Those of the older generation were encouraged to include personal reminiscences whenever appropriate.

Although the focus here is restricted to the anthropology of the socialist era, the chapters also provide some landmarks for understanding postsocialist developments in the field. However, evaluations of the scientific heritage of socialist anthropology remain burdened by the fact that memories of this era are still heavily laden with emotions and, at any rate in comparison with the former German Democratic Republic, no real coming to terms with communism has yet taken place in these countries.

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Vintilă Mihăilescu, Ilia Iliev and Slobodan Naumović (eds.)

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Preface

Our first effort at the Max Planck Institute to excavate the history of anthropology in the socialist era was restricted to the former German Democratic Republic and Czechoslovakia, Poland and Hungary. The resulting volume, published in 2005 as Volume 8 of this series, has generated a lot of attention in the countries it covered and elsewhere. As indicated in its title, the present volume is a direct continuation of that initiative.

There is no need to repeat here in detail the rationale that I outlined in my Preface to the 2005 volume. Since we at the MPI hope to continue to conduct research projects in the ex-socialist countries of Eurasia, and since we believe in working closely with local partners whenever this is possible, it behoves us to take an active interest in the recent history of our common intellectual community. Our efforts should not to be confused with the general logic of *Vergangenheitsbewältigung*, i.e. the necessity of coming to terms with the more problematic elements of the past, in order to be able to achieve a kind of catharsis and move forward on morally more secure foundations. Few ex-socialist countries have matched the rigour of the Germans in this respect, in any domain of political or academic life. We noted in the 2005 volume that only in the German case were the old institutions for research and teaching in anthropology systematically broken up and their staff made redundant. The countries covered in this volume fall much closer to the former Czechoslovakia, Poland and Hungary in that the demise of socialism has been characterized by a high degree of continuity in our academic field, both in structures and personnel.

Of course throughout Eastern Europe there have been changes and lively debates in recent years, some of which now have a significant impact on how the socialist era itself is perceived. Many readers may question my prior assumption of common intellectual ground. I continue to find it useful to define anthropology very broadly: it is a large umbrella term, under which socio-cultural anthropology, nation-focused ethnology ('national ethnography'), folklore, material culture etc. can all find a place. But I am well aware that some scholars would prefer to introduce socio-cultural anthropology as a new discipline, to complement or even to replace the nation-focused traditions established before socialism, which generally became more securely entrenched in the socialist era.

Whatever one's position in these on-going debates, greater awareness of disciplinary practice and theory in that era should make it possible for them to proceed more fruitfully. That is the basic assumption behind this collection, which derives from a conference at the Max Planck Institute in June 2006. Vintilă Mihăilescu played the leading role in the convening of that meeting and in the editing of the papers, assisted throughout by Ilia Iliev

and Slobodan Naumović. All three have been influential *Grenzgänger* in their respective countries. The task was not facilitated by the political fragmentation of the Western Balkans since 1990, which far exceeded the changes that took place in East-Central Europe. It was originally my hope that the convenors would be able to include all of ex-Yugoslavia and Albania in their coverage, but this proved impossible within the confines of a single conference.

As with the East-Central Europe volume, here too the reader will find an eclectic mix of contributions, ranging from historical overviews by the editors to analyses of specific institutions and individuals, personal reflections by influential actors, and more distanced appraisals by scholars of a new generation. The major innovation of this volume in comparison with its predecessor is that we also hear the voices of some distinguished North American anthropologists who interacted more or less closely with 'local scholars' in the course of their field research in the socialist era.

It remains for me to extend a warm thanks to all the contributors and to express the hope that this volume will not be the last of its kind that we publish in this Halle series.

Chris Hann

October 2007

Acknowledgements

The present volume is a follow-up to an earlier book on anthropology during socialism in eastern Europe, published in the same series. The initiative belongs to Chris Hann, the general editor of the series, whom the editors would like to thank for all his help and perseverance. Special thanks are due to Linda Woodward and Berit Westwood, who showed great patience in assisting us with their professional advice in the final editing process. We are also grateful to our colleagues Alexandar Bosković and Sorin Gog, who helped out with some editing problems. Last but not least, we would like to thank the whole team at the Max Planck Institute for Social Anthropology in Halle, who made our work possible and tried hard to make it also pleasant.

The editors

Chapter 1
Introduction

Vintilă Mihăilescu, Ilia Iliev, Slobodan Naumović

> The nation has been an inseparable accompaniment of modern European history. It is not difficult to ironize over the record of 'nationalism' in past and present, to criticize its role and to award good or bad marks to different groups, personalities or even nations, in the process. There is a public that finds this procedure to its taste, but it is not to be confused with a scientific approach to the subject.
>
> Miroslav Hroch, 2000: 78

The editors of the preceding volume[1] start their presentation with the following statement: 'We are anthropologists, not historians. Although we are concerned in this volume with the history of anthropology, we have in a way approached the task ethnographically…' As we are concerned with the same kind of history of anthropology[2] as our predecessors, but in South-East European socialist countries this time (Serbia, Romania and Bulgaria), we decided to 'approached the task ethnographically' too, i.e. to treat the local anthropologists as kinds of 'communities', deserving a proper, first-hand ethnography.

After studying the texts that were written for this volume, we understood that the central question uniting them was how locally differing versions of a specific type of intellectual endeavour – national science – functioned under three formally internationalist, but in substance deeply national, versions of socialism. In other words, we agreed that the book should initiate reflection on how various aspects of three respective nations were researched

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[1] Chris Hann, Mihály Sárkány, Peter Skalník (eds.), *Studying Peoples in the People's Democracies. Socialist Era Anthropology in East-Central Europe*, Halle Studies in the Anthropology of Eurasia, vol. 8, LIT Verlag, Münster, 2005.

[2] We will use 'anthropology' in the same rather loose sense of a conceptual umbrella as the one proposed by the editors of the first volume in order to overarch the different national and historical appellations of the discipline(s). Nevertheless, we have to remind the reader that the very term 'anthropology' is rather unusual – and rarely used until recent times – in the countries discussed in this volume.

and remedied under three versions of national communism. It was then that we decided to structure this volume into three country case studies.

But is such an approach legitimate at all? Is it acceptable to describe Serbian, Romanian or Bulgarian 'anthropologists' within such a restrictive frame of their 'own and only' national space? Are we not turning the wheel of anthropological reflection backwards, are we not additionally nationalizing national sciences by enclosing thematic cases within national borders? Are we not reproducing national science, instead of analysing it?

We believe that, at least to some extent, our risky approach can work, and even prove to be enlightening. Our central argument in this respect is that in all three countries 'home anthropology' – or what the Arnhem 1955 conference more appropriately suggested calling 'national ethnology' (Hofer 1968) – was in fact a 'nation-building' (Stocking 1982) science or, as most of the local scholars would call it, a *national science* (e.g. Gusti 1934), both in the pre-communist and in the communist period. 'Nation' was thus the *longue durée* political context and epistemological frame of reference of all these 'national ethnologies', urging whoever is approaching the history of the discipline to take this perspective into consideration.

But is this true for the communist times and their dominant internationalist ideology too? In fact, in all these countries of the Soviet belt, 'a second generation of "national Communist" reformers replaced the first generation of orthodox Stalinist leaders' (Johnson 2001: 190). With them, proletarian internationalist commitment was abandoned, and national interests returned to centre stage. In Yugoslavia, in order to tackle fierce national antagonisms, the communists resorted to a revised version of soviet federalism, which, from the late sixties started turning party elites from each republic into fervent guardians of their respective nations. In Romania the relationship between communism and nationalism had a different initial dynamic, but developed under Ceauşescu into what was considered by some scholars as 'the most eccentric form of national Communism' (*idem*: 191). In Bulgaria, a shyer version of this blend has emerged since the late 1950s, because the country remained a loyal satellite of Moscow and there was only a limited degree of nationalism that could be afforded in such a situation. Even if differently instrumentalized, *national communism* was a trans-border socialist phenomenon. Its emergence in the socialist countries called for a new look at the national agenda and refuelled 'anthropology' as a science deeply concerned with these national issues.

In all these countries, this very concept of nation oscillated between exclusionary, discriminative approaches (especially useful in the internal struggles between competing elites) and integrative approaches, often related to forceful inclusion of social and cultural minorities and subsequent ho-

mogenization. Sometimes it even changed its name, eventually its social space, but not its reason. In deep and complex ways, 'nation' was thus at the heart of the discipline and of the identity of its practitioners much earlier as well as long after the communist takeover – and, to some extent, it still is. Approaching the 'communities of anthropologists' and their 'cultures' as 'national' ones should thus not be considered an inadequate 'isolationist' procedure, but rather as an enlightening emic endeavour.

Having agreed on the central problem to be addressed in our volume, we further wanted to supply such a structure in which the thematic texts would communicate between themselves as intensively as possible, and thus provide a clear and consistent picture of the units that could be compared further. Each country case-study could thus follow its own inner logic and coherence without preventing from inter- and trans-national comparisons.

Anthropology's Coming of Age in Serbia, Romania and Bulgaria

Proceeding as anthropologists also means that we will not try to cover an historical overview of the subject – although such an approach would be extremely useful in our countries. Nevertheless, it is precisely as anthropologists that we intend to place our subject in its *longue durée* context, being convinced that limiting 'anthropology during communism' to its communist context would be (to risk being) misleading. We have thus to trace 'anthropology' back to its 'national' beginnings and to (re-) integrate 'communist anthropology' in this history.

The efforts to reflect on various aspects of what was to become anthropology, accompanied by a more or less systematic collection of ethnographic data can be traced to the very beginning of the nineteenth century. Yet, the professionalization and institutionalization of the discipline started somewhat later, towards the end of the nineteenth and the beginning of the twentieth century.

In Serbia, institutionalization of the discipline can be traced back *de jure* to January 1880, when comparative geography and ethnography were introduced as obligatory courses at 'The Great School' (*Velika Škola*) in Belgrade (later Faculty of Philosophy), and *de facto* on 3 January 1885, when the inaugural lecture of the 'Comparative Geography and Ethnography' course was held at the same institution. The process lasted approximately four decades, its key moments being those listed in the 'Timelines'. Institutionalization was completed only in 1922, after the devastations of the First World War were remedied, with the transformation of the Chair into the Department of Ethnology. Once established, the Department of Ethnology of the Faculty of Philosophy, unlike many similar institutions throughout South-East Europe, functioned continuously (apart from the tragic years

during World War II) to the present day. In 1947, one new institution was added to those already existing – the Ethnographic Institute of the Serbian Academy of Sciences and Arts. Together with ethnographic sections or regional complex museums, like The Museum of Vojvodina (*Vojvodjanski muzej*), and a number of chairs, independent courses, and research centres at various Serbian universities (the largest ones being the Universities of Belgrade, Novi Sad, Niš, Kragujevac), the listed institutions continuously offered, and still offer the framework of professional activity for the discipline of ethnology/anthropology in Serbia.

In Romania, after a long list of scholars concerned during the nineteenth century with the ethnogenesis of the Romanian people and its cultural landmarks, the first 'professional' survey was the huge questionnaire launched by Bogdan Petriceicu Haşdeu in 1877, followed in 1893 by another, elaborated and distributed by Nicolae Densuşianu, both covering large parts of 'traditional life'. But the real institutional establishment of the discipline starts with the beginning of the twentieth century. Between 1905 and 1910, the two main ethnographic museums were established in Sibiu and in Bucharest, Ovid Densusianu opened his leading philological school in folk studies, Simion Mehedinți inaugurated a course of Anthropography and Ethnography within the Department of Geography at the University of Bucharest, and the Romanian Academy started a monumental series of publications on *The Life of the Romanian People* (*Din viața poporului roman*). The emergence of Great Romania after the First World War brought a new impetus to anthropology as a national science amongst others – especially sociology, represented mainly by the important and powerful school of Dimitrie Gusti. A department of ethnography opened at the University of Cluj in 1926 and the first courses in folklore started in 1928 at the University of Bucharest, the two disciplines developing quite independently. With the takeover of communism, most of these institutions and trained scholars had to stop their activity, some of them for good. New institutions were built and new political claims were made, orienting the discipline mainly toward the 'new folklore' of the proletarian people. But this political trend did not last very long – and did not totally move anthropology away from its traditional interests. As a matter of fact, it officially returned to folk culture and traditions in the sixties, following Ceauşescu's independence movement toward Moscow and growing national communism. Important academic projects were (re) started (the corpus of the Romanian folklore, the ethnographic atlas, etc.) and continued throughout communism. A genuine 'folk-production' also started under strict party control but with the help of 'folk experts'.

In Bulgaria, the discipline was institutionally established ten years after the foundation of an independent Bulgarian state. In 1889 Ivan Shismanov published *The Importance and the Tasks of Our Ethnography*, arguing in favour of a comprehensive *narodouka* (literally 'study of the nation'), encompassing ethnography and folklore, and investigating the national traditions in a comparative Balkan and Slavic perspective. The same year a course on comparative Slavic Ethnography was launched at the University of Sofia. In 1906 a National Museum of Ethnography was established, providing a base for research and collection. The teaching was situated mostly at the Department of Slavic Languages and the lasting interest in comparative Slavic study, implying an analysis of the common characteristics and origin of the Slavic nations, guaranteed a reputation of Slavophiles to the professors there. This largely facilitated a process of relatively smooth transition after 1947, when the university chairman of ethnography became the first director of the newly established institute of *Narodouka*, which inherited the institutional structure and the staff of the National Museum of Ethnography, and was later renamed the Institute of Ethnography and Museum. However, several ethnologists (like Mikhail Arnaoudov, discussed in this volume) suffered various persecutions. Much like their Romanian and Serbian colleagues, the Bulgarian ethnographers of the late 1940s and 1950s had to focus their efforts on investigating the culture of the working class, youth movement, and the few collectivized peasants. These were the three main categories of population worth studying from the party's point of view. In addition, they were expected to supply important expertise in the efforts to modernize the socialist state. Again like the Romanian case, in the late 1950s the Bulgarian ethnographers were encouraged to return to their former fields of interest. They renewed their studies on traditional folk culture, with much the same intellectual tools, but in a new context. In 1972 the Institute of Folklore was established, a rival twin institution to the existing Institute of Ethnography.

Romanticist Original Sin…

'The *locus classicus* for an intellectual declaration of an "eastern" model of nationalism is the oeuvre of Johann Gottfried von Herder', Chris Hann reminds us (2006: 400). In the same vein it is commonly considered that 'ethnology' in all these countries was from its beginning – and because of this beginning – *nationalist*, descending from the same German intellectual ancestor. Whilst he was not a romanticist himself, Herder strongly influenced – with his 'ethnic' ideas about the *Volk* and its *Geist* – the political romanticism of the 'young nations' and their founding fathers. Indeed, in the case of Yugoslavia, for instance, the romanticist intellectual heritage of its

most influential predecessor, the linguistic reformer and folklorist Vuk Stefanović Karadžić, further developed in the theoretical conceptions of two other romanticist-inclined minds, the legal scholar Valtazar Bogišić and geographer, geologist and anthropogeographer Jovan Cvijić, and reinforced by tumultuous historical developments and related social expectations, pushed Serbian ethnology in the direction of a national science with substantial applied tasks. In Romania and Bulgaria the final touch of the national agenda was deeply inspired by romanticism too, further influencing the establishment of anthropology in these countries as a *Volk*-centred, national science. Looking at the Croatian case, Olga Supek – to take just one example – sums up the common fate of the discipline in the following way: 'since ethnology in Croatia sprang from an interest in self-discovery and self-legitimization, it developed primarily as a "national science", with an acute need for conceptual tools to grapple with the analysis of countries. Hence, rather than being defined as "the science of culture" as in imperial Great Britain in the age of Tylor or the burgeoning USA in the age of Morgan, it was formulated as "the science of peoples", i.e. *ethno-logy*. *Ethnos* became the focal concept: though initially in ancient Greek the term basically meant "belonging to a group", in ethnology the concept came to signify "the essence of a people". The essence was presumably a natural, genuine, inherent quality, entailing an almost mystical link between a people and the soil on which it lived. The common and unalterable nature of *ethnos* was not questioned – the task of an ethnographer was merely to document manifestations of the peoples' "soul", be it in folk art, myths, beliefs or ritual' (Supek 1988: 17–18). Even the history of the discipline was – and still is – conceived in such a way as to suit its supposed perennial national mission. It seems thus that the romantics had a firmer embrace and overtook the better part of the discipline in all these countries.

Based on such evidence, the 'unilinear descent theory of ideology in ethnology' (Naumović 1998) suggests that by inventing the modern *Volk*-centred conception of nation, and setting it on the road to political independence *via* nationalism, romanticism sentenced the not-yet fully conceived science of ethnology to a sudden and unwanted birth, and to an undignified life of intellectual servitude. This view is rooted in the more familiar of the two types of nationalism within Europe, popularized by Hans Kohn and others, 'Eastern' countries typically sharing the 'ethnic nationalism' of Herderian obedience. A further step was to ascribe to the countries this defining type of nationalism (and national sciences) to precise geopolitical spaces, as suggested, for instance, by Claude Karnoouh: 'it is only North, East and South-east of the Rhine that ethnography and folklore (alongside philology and history) were used as scientific proofs in the service of poli-

tics, and more specifically, as ground for the political discourse of origins' (Karnoouh 1990: 141). This logic may reach a climax when some critics take over the very incriminated romanticist essentialism in order to describe the peoples they are criticizing for using it, as in the case of Marek Kohn: 'East-central Europe is still a Romantic landscape,' he claims (Kohn 1996: 193), where 'the democratic traditions have very shallow roots and the populist ones are part of the cultural patrimony' (*idem*: 188). To phrase it in another way, this means that Eastern European scientists are national essentialists because they are romanticists, and they are romanticists because they are living in Eastern Europe (North, East and South-east of the Rhine, as Karnoouh would say) and this is their 'cultural inheritance'. Or, in a still different way, this would mean that these societies had chosen the romantic ideology because they were already romantic in their social being. Indeed, it seems that it is not possible to escape romanticist original sin in this part of the world!

As a matter of fact, sometimes and in some cases it is difficult indeed to escape a kind of romanticist flavoured traditionalism deeply rooted and preserved in lay and academic beliefs and routine. But does this mean that romanticism should rightfully be accused of being the principal inspiration of 'sinful' – i.e. ideological – behaviour of ethnology? Is the answer really so neat?

…and Beyond

Let's go then back to Herder, the scapegoat of all the ideological blames for 'ethnic nationalism', and have a closer look. Beyond the standard and sometimes rash statements, it may be worth recalling Herder's original position and why it inspired, to such an extent, the young nations' revolutionary pathos.

Herder's main 'principle', considered as such – and rejected – by his teacher, Immanuel Kant, can be summed up in the following sentence: 'The One who gave man reason, gave it within language.' Embedded in the vernacular language of every given population, the Herderian reason turns thus to plural. From the very origins and for ever. Reasons instead of Reason, particular instead of universal history, communities instead of individuals, are only some of the most important consequences of this original standpoint about this *sui generis* reason of each community of language, the *Volksgeist*. It is not by chance that Herder has chosen *Proteus* to symbolize this diverse and changing human world.

From a political point of view, this originates in a strong reaction against the hegemony of Enlightenment and will give birth in time to the alternative and more down to the earth 'rights of peoples or cultures'. In his

celebrated essay, Louis Dumont resumés Herder's contribution and originality as follows:

> against the ruling universalism, Herder is passionately claiming in 1774 the diversity of cultures … One may state that here we have a beforehand alternative to the future rights of man: the coming rights of cultures or peoples … We are facing here an essential difference in the way of approaching man: either the fundamental value is placed in the individual, and then we will speak about the individualism of Voltaire or the Encyclopaedists, or it is placed in the society or the culture, in the collective being, and then I would speak about the holism arising in Rousseau and in Herder … [For Herder] the cultures are collective individuals (Dumont 1983).

It is mainly this spirit that most South-east European 'young nations', having to define and build themselves against powerful surrounding empires, will celebrate and put to work for the sake of their own *Volk*.

Additionally, Herder is a forerunner in prizing the *peasant Volk* for its poetry, i.e. as an aesthetic subject, and thus as an historical subject *in nuce*. All the nations we are dealing with will have to do something similar in order to turn their large peasant populations into historical subjects of their modern fate.

Another common ideological reference is the German *Volkskunde*. It may then be worth taking a closer look at this founding discipline too.

> As we know it for sure now, the real definition of *Volkskunde* refers, since the XVIIIth century when the word appeared for the first time in the context of administrative statistics, to the 'knowledge about the people' (*Kentnisse über das Volk*) and not to the 'traditions preserved by the people' (*Überlieferungen im Volk*) (Brückner 1987: 228).

Hence, it seems that *Volkskunde* was initially brought into being by the needs of political and administrative modernization. The last – and by far better known – meaning appeared only later on and turned to a mainstream under the influence of 'romantic literary ambitions and the emergence of a national historiography' (*idem*). Nevertheless, the local/national customs (*Sitten und Kultur*) were shared as an object of interest by both approaches, only from rather different standpoints. Both meanings – and practices – continued together for a long time, even if in unbalanced ways sometimes. And both were linked, in their own way, to the same needs of modernization and state and nation building.

We may find thus in all our three countries, in the first half of the nineteenth century and in the context of the changing power relations of the surrounding empires and emerging nation, a growing interest in general data

about the people of these territories for diplomatic, administrative and/or economic reasons, a growing corpus of useful administrative and economic statistics and geographic descriptions. These 'sociographers' (Stahl 2001) were practising mainly a kind of *Volkskunde* in its original sense of 'knowledge about the people', usually ordered and used for state development interventions and thus having to be included in the modernization projects of the region.

In Bulgaria, for instance, the first systematic ethnographic and folkloristic research of Bulgarian culture was conducted by Yurii Venelin, a Russian subject, sponsored by the Russian Imperial Academy of Sciences, interested in the almost unknown Slavic tribes south of the Danube. Ethnographic interests were thus rooted from the very beginning in a comparative approach of the Slavic peoples. In Transylvania, systematic ethnographic interest starts with the rationalist Enlightenment movement of the 'Latinist School'. In this case, the comparative approach was less important, Romania perceiving itself as an 'island' of Latinity and thus less interested in looking for comparisons with closer or more distant neighbours. In a typical Enlightenment vein, knowing the peasants' customs was important in order to dismiss the lasting *superstitions* rather then for the sake of describing and preserving *traditions*.[3] In Serbia, 'ethnology' was unambiguously defined as a comparative and theoretical scientific discipline in the key programmatic texts from the period of its institutionalization. Comparative or not, the discipline is initially motivated mainly by the need of first-hand documentation about people's lives.

But why then give up this approach in order to take the *Herder connection* rather than the *French connection*? Why take over the *Volkskunde* kind of approach(es)? What inspired and made preferable this particular type of national commitment? The answer has to be sought, as usual, in the interplay of political stakes and socio-economic means of an historical context, even if – or especially because – what has to be explained at the end is an 'ideological process', as suggested by Katherine Verdery (1990: 81).

In this respect, all we can do in this brief introduction is to repeat some features of this context that have been defining for the 'national movements'[4] and of a particular importance for the process of nation-building anthropology in our countries:

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[3] The very title of a representative work of this school is telling in this respect: 'Reasonable Teaching to Sap the People's Superstitions' (*Învățătură firească spre surparea superstiției norodului*), by Gheorghe Şincai 1983 [1808].

[4] In this respect, it is worth remembering – in order to avoid – the confusion Miroslav Hroch was pointing at: 'the process whereby nations were formed in Europe is typically represented as the unfolding or spread of ideas of "nationalism"… In my view, this is a basically mislead-

a) Because of the specific constellation of historical factors (mainly the collapse of local feudal states and the incorporation of the remaining ethnically and culturally unhomogeneous agrarian populations into large culturally distinct empires), the nation these societies were seeking was rather a *unified*, *collective subject of independence* and sovereignty than an *individualized subject of liberty* (Mihăilescu 1991). The 'invention' of such a national collective subject was thus a must of the national movements, beyond and before any considerations on what the 'national character' of this subject could or should be, and eventually with no links to nationalism.

b) Demographically, this collective subject had to be the peasantry, or at least to incorporate in one way or another these rural populations that were by far the largest category in our societies. In all these cases, the nation had to be built not *against* the local peasantry, as claimed by Ernst Renan in his celebrated essay about the nation (1887) for instance, but *together* with this peasantry. Or the dominant model of the peasant, from the French Enlightenment to Karl Marx, was that of the *inner primitive*, 'the view that the peasant is the antonym of progress', 'a developmental cul-de-sac' (Leonard and Kaneff 2002: 7). Obviously, such a view was not very useful when having to modernize and develop an agrarian society and turn it into a modern nation state. In order to manage this, one had first of all to change the image of the peasant, to turn this 'backward peasantry' into a praiseful *national Peasant*. Here is where the Herderian view fits in, covering, without many adjustments, this essential ideological and symbolic need of backward agrarian societies turned intomodern nations.

c) And here is also where the 'national ethnologies' had to intervene in order to shape this national peasantry: being a 'nation-building' discipline, anthropology had to be in our countries first and foremost a *peasant-building* discipline. It was similar to the way colonial em-

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ing way of looking at the subject. For the diffusion of national ideas could only occur in specific social settings. Nation building was never a mere project of ambitious or narcissistic intellectuals, and ideas could not flow through Europe by their own inspirational force. Intellectuals can "invent" national communities only if certain objective preconditions for the formation of a nation already exist. Karl Deutch long ago remarked that for national consciousness to arise there must be something for it to become conscious of' (Hroch 2000: 79). In order to overcome this confusion, Hroch makes a clear distinction between 'nationalism' *stricto sensu* and what he calls 'national movements'. In this view, he terms 'these organized endeavours to achieve all the attributes of a fully-fledged nation (which were not always and everywhere successful) a *national movement*'. On the other side, *nationalism*, he argues, is 'that outlook which gives an absolute priority to values of the nation over all other values and interests' (*idem*: 80, 81).

pire-building anthropology had also to be a primitive-building anthropology. Similar but distinct: colonial anthropology was linked to the problem of *ruling over* distant *primitives*, while national anthropology was involved in the problem of *ruling with* fellow *peasants*. Anthropology in these countries is thus not just a domestic version of anthropology: it emerged from a distinct stake and approaches a distinct object then Western colonial anthropology.

d) This 'nation', having the *peasant* as its main actor and symbol, 'refers to the ethnic idea of *people*' (Verdery 1995: 105) so that the lasting question is not *what is the Nation*? but rather *who is the (real) People*? Initially identified – not without conflicts and polemics – with the peasants or, more generally, the rural people,[5] this *Volk-Nation* was a constant object of concern, its very notion shifting content from one category of 'defenders of the nation' to the other and from one period to another. Thus, socialism had to dismantle the 'bourgeois' concept of people, shifting first to the notion of the proletarian people, and later on preparing the future of the classless communist people. During all these metamorphoses, the peasantry was still a large and meaningful category with which one had to deal. Anthropology, as an expert in the 'Peasant and People', always followed these shifts and was, to some extent, part of them.

e) In this context, it seems useful to analytically separate *state building* from *nation building*, as suggested by Katherine Verdery (1990: 82). And this especially because, as noticed by Verdery in the Romanian case, nation defending institutions may work successively as well as simultaneously for as well as against state institutions and strategies. Thus, anthropologists, as 'national scientists', were not necessarily the apologues of *the* Nation, but were always *the* experts in all sort of national identity issues, thus having to be taken into consideration by the State and preserving throughout time a power position. In the case of socialism, this power game also embraced a form of double talk by staging a kind of pre-communist, eternal 'nation' and pretending that it just follows the Communist Party-state's nationalistic orientation. For many scholars, this kind of compromise was considered a smart way of coping with the system – and some of them were convinced that it even meant fooling the system.

[5] The Romanian ethnographer and historian of the discipline Ion Muşlea points to the way the national elites were rather trying to wipe out or at least to diminish the differences between free peasants and boyars in order to sustain the unity of the national culture as an expression of a generic rural people (Muşlea 1971 [1935]).

f) The image of – and belief in – the backwardness of the peasantry was never entirely dismissed by its romantic counterpart, and the modernization efforts of the national states could never entirely overcome the real backwardness of their rural worlds. These societies were thus at their best semi-successful 'latecomers' of modernity and permanent 'not-yet-European' nations (Plamenatz 1973). This further created a burning feeling of *resentment*, and led the local intellectual elites to consider romantic nationalism the viable strategy of overcoming the historical problems their group was faced with as well as the derived feelings of national inferiority. Romantic ideology thus turned from a means of the national movements to more or less explicit nationalism.

g) The most important feature of the *resentment*-driven 'Eastern' nationalisms was their *paradoxical nature*: in order to preserve their cultural identity, that is their sense of nationality, in the face of military, economically, and culturally overpowering masters or threatening neighbours, they had to imitate the oppressors or potential aggressors they hoped to overcome, and in that way undermined their own anchorage in the way of their fathers. Thus, there was a double sense of ambivalence to overcome, stemming from two rejections to be made – that of the *foreign dominator* for which the only way was his overcoming by standards he himself introduced, as well as that of the *traditions of the ancestors*, which had to be cherished as symbols of identity, but had also to be considered as obstacles to the desired future of modernity and, eventually, full European belonging. Anthropology, again, followed these states of mind and was, in different ways and degrees, part of them.

In this light we perhaps understand better why it happened that the *Romanticists*, and not the *Rationalists*, were the flagship of modernization and further on the ones to mould the intellectual identity of anthropology in the Balkans. More specifically, we may see how the specific context we have just tried to sketch created a specific situation, in the frame of which Enlightenment approaches were not necessarily opposed to the romanticist influences but conflictingly cooperated for the sake of modernity and produced these *sui generis* mixtures[6] some like to call 'ethnic nation'. And we may also under-

[6] A representative such 'mixture' is the idea of development as *improvement* or perfectibility instead of *evolution*. The credo of Ion Ghica, a leading figure of the Romanian nation-building Pantheon, may be enlightening in this respect: 'the most beautiful of all the laws of the universal realm (is) the law of *perfectibility*', he claimed (Ghica 1914: 122). This view was shared both by Ion C. Brătianu, the founder of a prominent dynasty of liberal leaders and one of the fathers of the modern Romanian state, and Mihai Eminescu, the romantic national

stand why romantic love for the nation's soul and body just could not be the only 'engine of action' on the way to modernity, and why the still dominant theory of unilinear Romantic-Herderian descent of ideology in anthropology is not sufficiently comprehensive.[7]

One could say, therefore, that modernization and Europeanization were on the agenda of anthropology as a national science simultaneously with the more romanticist tasks of collecting and preserving folklore and peasant culture in general. During the 1950s, when institutional anthropology was constrained to work to build the proletarian *Volk*, it was again about modernization.

Anxiety about state independence and sovereignty, resentment about backwardness and paradoxical strategies to overcome them, all these 'life conditions which produce ideologies' did not disappear in communist times. Neither did the ideological tool-box produced over time in order to face these problems. Fearing Moscow internationalism or Serbian nationalism and/or unitary Yugoslavism, opposing Western economic superiority and fearing one's own economic backwardness have also been reasons for new national turbulence – eventually new 'national movements' aiming to build new, communist, nations. It is in these old and new contexts that national ideologies had to be revisited and the national component of (some) social sciences – anthropology in the first place – were refuelled.

The Double Insider Syndrome

Not only was the political context of national movements and nationalism a specific one, producing the need of a 'national science' with changing faces, but also the place and role of the 'national scientist' in this context had its particularities. Compared with their Western, 'empire-building' colleagues, the local 'nation-building' anthropologists had – and sometimes still have – a specific *existential position*. That is, they consider themselves as belonging to the group they study, share its language, traditions, dominant values and political interests. Consequently, the anthropologist is not facing, in this case, the famous anthropological *distance*, to be overcome by the long-term participant observation. On the contrary, he is supposed to share from the very beginning the 'native's point of view', the problem being how to overcome this axiological and emotional proximity, not how to carefully build it,

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poet and an influent conservative politician. We may find versions of this view at other times and in other places of the South-Eastern European countries.

[7] As noticed by Chris Hann, it is only recently that this 'dichotomy between Western and eastern forms of nationalism, popularized by Kohn' was put under scrutiny by some scholars who 'begun to draw attention to the extent to which it misrepresents nations and nationalism in both East and West (Kuzio 2002)' (Hann 2006: 402).

how to distance him/herself from his/her fellow informants in order to better understand them and not how to get closer to them (Iosif 2007). Simultaneously, they perceive themselves and are publicly perceived as belonging to the intellectual elite of their nation whose socially defined task is to study, consolidate, invent, and eventually defend the 'cause' of the larger group (and which consequently has its own record of alliances and rivalries with other national elites). It is important to note that both the *insider*-situations are potential factors of ideologization of ethnological discourses. The only difference between them lies in the fact that the anthropologist can in the first case be engaged in an ideologization of which he himself is not fully aware, while in the second, she or he ideologizes her or his discourse deliberately. These two types of ideologization are not mutually exclusive. On the contrary, a combination of their effects is the probable outcome in the countries with which we are concerned. It is precisely this mutually stimulating combination of unconscious and intentional ideologizations that was termed the 'double insider syndrome' (Naumović 1998). Thus, the unconscious bias that threatens the ethnologist doubling as native, the need for a calculated fact-twisting approach that usually goes together with the role of the ethnologist as his group's advocate (an imagined group that he should both construct and defend), merged with the burning desire to rapidly overcome the 'unbearable and undeserved' position of the society as a whole, characteristic of the anthropologist as object and subject of history, conspire together to produce a fiery blend, the *anthropologist as ideologue of the Nation*. Whatever the particular meaning of this 'nation' would be.

It is probably this same 'double insider syndrome' that explains – at least to some extent – the fact that in none of these Balkan countries has anthropology gone through a post-national critique similar to the post-colonial one the Occident went through (Todorova 1997). Local anthropologists were concerned with the Peasant instead of the Primitive, and, in the prevailing romanticist view, this Peasant was not a disregarded inner Primitive but a loved and loving national fellow for whom the anthropologist was supposed to fight. Why then spoil this love affair between the anthropologist and his subject of research by an un-needed critique? Further on, this makes any critical approach of the national histories of anthropology in general a very sensitive issue: to have a distant look on your own biased work as defender of your fellow subjects of research is not an easy game! During socialism, one should add to these difficulties the troubling problem of (possible) double discourse of the anthropologist we have already referred to. And now, another obstacle has to be taken into consideration: the still trouble waters of the social memory of communism that systematically

affects the memories about the way anthropology was practised during that times too.

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We had to face these problems too when working on this volume. First of all, we had to handle the critical lack of previous research, especially in the case of Romania and Bulgaria. Then we had to cope with differences in the *existential position* of our colleagues as well as ourselves and the different ways all of us have been mastering our 'double insider syndrome'. Differences due to generation gap and ways of institutional implication, especially, soon became obvious. Some of the papers were thus closer to critical reflexivity; some were rather kinds of personal testimonies about the state of the art. Some were careful overviews embracing almost the entire period of communism, and thus serving as opening chapters (Otilia Hedeşan, Ilia Iliev and Slobodan Naumović). Some focus on representative figures of the discipline (Milena Benovska-Sabkova, Anelia Kasabova, Gordana Gorunović, Ivana Spasić), while some other preferred to describe the discipline by focusing on emblematic institutions (Vintilă Mihăilescu, Corina Iosif, Vasil Garnizov, Marina Cvetković, Mladena Prelić). But, as already mentioned, all deal, in one way or another, with the problem of 'nation' and self-position themselves in this respect. We thus decided to make a place for all, being as balanced as possible in order to cover as many points of view as we could. Finally, we also decided to write this brief introduction as a sort of shortcut to compensate for still missing pieces of critical research, and to ensure that our chapters are approached in what we consider to be their proper perspective: the *longue durée* of national movements, nationalisms and nation-building anthropologies.[8].

It is with the same perspective and for the same reasons that we decided to solicit opinions from some Western anthropologists who worked systematically in our countries in this period. Their view was considered to offer an insightful complement to the analyses of our national colleagues. The last part of the volume is thus dedicated to three interviews with Western anthropologists playing the role of the *representative Other*. In addition to the interviews conducted by Vintilă Mihăilescu with David Kideckel, Chris Hann with Carol Silverman, and Mirjana Prošić-Dvornić with Joel M. Halpern, the chapter by Asen Balikci in Part Two also includes 'some critical comments' on the knowledge production of the socialist era.

[8] For this reason we also considered it useful to provide the reader with some timelines of the national contexts and the national 'becoming' of anthropology in Serbia, Romania and Bulgaria, see the Appendix.

In doing so, we are aware of the fact that what we offer the reader is just a draft portion of a still missing whole.

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PART ONE

ROMANIA

Chapter 2
Doing Fieldwork in Communist Romania

Otilia Hedeşan

'We went there … what did we see? I do not know if it was something obvious, it was the first time that we took part in something like that. Anyway, we went there to do collective research on a village' (Rostas 2003: 110), confesses Marcela Focşa, remembering the starting point of fieldwork in inter-war Romania. By their rough expression, in the manner of an interview, these lines are almost emblematic for what fieldwork had represented for the great majority of Romanian ethnologists: a place that had to be covered, a space that had awoken an irrepressible interest, first of all due to its newness, a perimeter which is shaped as a research field even though what was at stake might have been uncertain for part of the team, at the beginning. Perceived most frequently as an 'interesting [practice], different from the letter of the book, [which allowed you] to meet life, reality' (Rostas 2003: 202), fieldwork was promoted as a cornerstone of a major part of Romanian social research from the very beginning.

The Traditions of Fieldwork in Romania

Any analysis of fieldwork undertaken in communist Romania, with the purpose of investigating traditional culture, cannot ignore the tradition of Romanian fieldwork from the period preceding the two world wars. At least three main methodologies and institutions need to be discussed for this purpose. I refer to: (1) Ovid Densuşianu's philological school, active since 1906; the director's monumental masterpiece, *Graiul nostru* [*Our Language*], contains texts obtained as the result of some of the author's 'voyages' in the field (Densuşianu 1906; see also Hedeşan 2005); (2) The Sociological School of Bucharest, headed by Dimitrie Gusti, whose main project (started in 1925) was a series of village social monographs elaborated through the direct observation/participation of researchers in the lives of the respective communities during campaigns that lasted for a few weeks and

involved large interdisciplinary teams (between 11 and 89 people)[1] (Rostas 2003: 13); (3) the *Archive of Folklore* of the Romanian Academy in Cluj, inaugurated in 1930, under Ion Muşlea's coordination. Its priority was the elaboration of ethnographic regional monographs (Muşlea 2005: 44) by multidisciplinary trained specialists' stages in the field[2] (most of them ethnographers and linguists).

What was the contribution of each of these schools to the general development of fieldwork in Romania?

Ovid Densuşianu was convinced that the only really interesting thing in folklore was the study of the *actual fact*, registered *in situ*, in its direct expression (Densuşianu 1966: 50). Promoting this idea, he firmly stated at the beginning of his university career that there was an urgent need for Romanian research to focus on these aspects. This approach recommended fieldwork as an indispensable preliminary condition of any kind of folk research and oriented fieldwork toward current facts of folk life. This openness of mind was restricted, however, by his own philological interest, to listening to the descriptions people gave him. Unfortunately, Densuşianu's interest in fieldwork was thus limited to the *texts* he might get there. Consequently, during their fieldwork, he and his main assistants produced extremely diverse texts, from variants of classical folkloric themes to fine ethnographical descriptions and fragmentary information on people's daily lives, on their contact with important personalities of the time, or on their reactions towards the representative events of the history.

Paying attention only to what people were *saying* – and especially to the *way* they were saying it – Densuşianu's closest co-workers actually ignored what people were *doing*.[3] Consequently, their fieldwork remained just a place where well-speaking informants might be discovered, but almost never a place where one might observe interesting and intriguing social practices. What the researcher had to do in this context was rather to *transcribe* what he was told, than to *translate* into a narrative – descriptive in itself of what he/she had observed.

A few decades later, after the First World War and the configuration of the modern Romanian state, Dimitrie Gusti gave another orientation to

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[1] The main 'monograph studies' were as follows: Goicea Mare, 1925, 11 participants; Ruşeţu, 1926, 17 participants; Nereju, 1927, 41 participants; Fundul Moldovei, 1928, 60 participants; Drăguş, 1929, 89 participants; Runcu, 1930, 67 participants; Cornova, 1931, 55 participants.

[2] See, in this respect, *The Yearbook of the Archive of Folklore*, I–VII, 1935–1945; each volume includes a regional ethnographic monograph, elaborated as a result of some field trips.

[3] This characteristic may be considered an early expression of the lasting *rationalist* preference for 'ideas' and 'what people are saying' as opposed to the *empiricist* interest in 'social interactions' and 'what people are doing' (Leach 1976), specific for a large part of Romanian social thinking (Mihăilescu 2003).

fieldwork. Sociology, as the very 'science of social reality', was aiming to help the development of the country, being thus attributed the status of a militant 'science of the nation'. In order to achieve this high-ranking mission, a careful methodology was designed and a huge institutional network was built. Fieldwork itself had to become more complex to fit into this national undertaking. Unlike Densuşianu's earlier 'voyages', the sociologists of Gusti's school developed an approach close to what we used to call *participant observation*: 'This is a long-discussed methodological issue', Mihai Pop, a late-coming member of Gusti's monographic teams and future director of the Institute of Folklore, remembers. 'In fact it is what the Americans call *participant observation*, that is an investigation you take part in but you do not intervene, allowing the events to flow, and note everything and afterwards, after you have written everything down, if you want to clarify something you do another interview with that person' (*apud* Rostas 2003: 285). A genuine 'switch from what people *tell* about themselves to what people *actually do*' (Rostas 2003: 289) was thus imposed by this sociological school. Beyond this innovation, Gusti also insisted that this observation should be pursued by complex, interdisciplinary teams made up of sociologists, demographers, ethnographers, rural economists, musicologists, folklorists, specialists in popular arts, etc. Fieldwork was thus given a *sui generis* collective dimension.

Finally, the group around the Cluj Archive of Folklore practised a kind of fieldwork in-between these two directions. As linguists, they overrated the importance of the narrative document, of the written text, an accurate text having to strictly follow the informants' words. Still, without programmatically practising *participant observation*, the researchers sent by the Cluj Archive on usually remote and almost 'exotic'[4] fieldwork, noted – even though fragmentarily – a series of personal observations, especially on everyday life and on the context of the research.

These few notes are intended to briefly sketch the heritage of fieldwork approaches anthropologists could – and had to – cope with during communist times. In this respect, there was a whole range of options, from the total rejection of methods and persons, especially in the Stalinist period, to a diplomatic 'restoration' of pre-communist authorities on the discipline.

[4] Ion Muşlea declares in the programme-article of the Archive's publication, *Yearbook of the Archive of Folklore,* that one of the main purposes of the institution is to produce monographs on not previously investigated regions of the country.

The Fifties

The takeover of communism affected folklore and ethnography studies in many ways. First, they were perceived as 'too national' in an era of Soviet imposed internationalism. Secondly, in Romania folklore and ethnography were traditionally associated with the peasants, initially suspected in the Leninist vein everywhere in the Soviet sphere of influence (Leonard and Kaneff 2002), and later on barely accepted as a secondary ally of the proletariat. Consequently, folklore and ethnography, in their original form, were also suspected – or had to adjust their interests to the new imperatives of the proletarian culture. Thus, the Institute of Folklore, created in 1949, initially hired exclusively musicologists and imposed, in the line of Soviet ethnography, the idea that folklore is by excellence art and especially musical art. In the Soviet line too, 'Creation Houses' were established in 1953, and started to collect and diffuse the *new folklore*, singing the 'successes of building socialism'. More often, it was in fact produced by activists in order to be transmitted as a model.[5] Last but not least, interdisciplinary fieldwork, as it was practised in the inter-war period, was no longer possible.

Beyond these ideological constrains, fieldwork in villages under the pressure of forced 'cooperativization', and deportation of wealthy peasants was highly improper and even risky in the early 1950s (for a detailed presentation of the period, see Erătescu in this volume).

Despite all these major difficulties, which can barely be appreciated today, some folklorists managed to do some kinds of fieldwork. I am referring primarily to what I would call the numerous 'field documents on given folk species' that some researchers produced during this decade. Practising a kind of 'folklore-hunting', more specifically, a type of strictly frameable folklore, within the limits of a well-defined genre, these researchers covered almost the entire Romanian territory in their attempts to record as many variants as possible of a genre, a theme or a folkloric motif. In doing so, all of them were following the same trajectory: the institute – the local authorities, which were converted *ad hoc* into an informal information bureau concerning the condition of folk culture in their region – the connoisseur of the genre or of the investigated subject.[6] As the only purpose of fieldwork

[5] In an interview with Nicolae Bot from June 2005, the Cluj folklorist recalls an episode that happened after Petre Ursache's book on the *New Folklore* was published. Surprised by the texts included there, some of them coming from villages that Nicolae Bot had been to, he went back into the field and tried to find them, too. He found out that either the respective informers did not exist or that they had never transmitted any such texts like thos published by Petre Ursache.

[6] See in this respect the interviews with Silviu Angelescu (November 2004) and Paul Drogeanu (November 2005), realized as part of the project called *The Practice of Ethnography*

was the meeting with this connoisseur in order to write down the information, the researcher was not interested in what might happen on the other side of his informant's door.

'The fruit-growing-metaphor on *collecting* the data … suggests that all the feast's ingredients were there, on the field, like fruit in a tree, waiting on a branch for the scrupulous folklorist to pick them up and put them in the basket of an archive' (Mihăilescu 2007: 84). Nothing more was suggested for this kind of 'documentary-trip', whose only stake was merely to collect and carefully preserve different elements that later on will recompose meaningful wholes. Everything around these elements, i.e. their context, was entirely neglected.

The most important representatives of this type of research were Alexandru Amzulescu and Ovidiu Bîrlea. The former published in 1964 an impressive collection of popular ballads, the latter published, two years later, a monumental prose anthology. Even the most superficial reading of these works would prove that in those times, while the research of social life or popular systems of beliefs was obstructed, research on classical folk species was still in good health.

As described above, these field trips seem to have been lonely journeys. But they were not:

> … we used to go, very often, with the cars of the Institute, and, in the cars, there we would transport some immense tape recorders and batteries would be transported, too, and some really big tapes, as they were big back in those days, and it would often be very cold, so the batteries needed to be warmed. There were any such technical 'slaveries' and this is why we would usually have two technicians, not only one and a driver. So there were teams that, even if they included only one or two researchers, were actually made of four, five, or six people.[7]

This account points also to the fact that the typical, *tête à tête* conversation between the researcher and his/her informant was not possible during these times. More importantly, behind this 'technical' scenario a political issue was often hidden: one of the members of this technical team was usually also an informer of the political police.

Other kinds of fieldwork were emerging in province universities such as Cluj or Timişoara. New folklore courses were introduced and their leaders were very young people, who had almost no 'past' at all, the majority of

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under the Communist Rule. The Romanian Case, supported by the Wenner-Gren Foundation and coordinated by Corina Iosif. All the interviews referred to in this article are parts of this project.

[7] Interview with Sanda Golopenția, May, 2004.

whom were politically desirable, even if they were not necessarily politically involved. Independently and intuitively, counting on the enthusiasm of the students (coming from the countryside, like themselves) and on the benevolence of local authorities, these very young teachers[8] organized the first large-group field inquiries. As Nicolae Bot recalls, nobody was very sure of what they had to do in such circumstances: what to observe, what to ask, what to note. 'We would ask, just like that: but can you sing, M'aam! And we would start talking, if she knew…'[9] Without committing oneself,[10] I consider that these field inquiries of the late 1950s, done together with the students, inaugurate the systematic and institutionalized practice of fieldwork, in the way it will be accredited and even confirmed by the authorities. They also *sui generis* re-connected present communist practices to the pre-war tradition of anthropological fieldwork. In this respect, after being appointed co-director of the Institute of Folklore, Mihai Pop claimed in his programmatic article in the *Review of Folklore*: 'At the beginning of the twentieth century, a new orientation arises in our folklore research … The folklorists want to ground their research on authentic material … gathered by them directly from the people' (Pop 1956: 7). The main researchers referred to in this sense were Ovid Densuşianu, Bartok Bela, Constantin Brăiloiu, H. H. Stahl, Ion Ionică and Ion Muşlea, all of them paradigmatic pre-war researchers. This politically correct 'new orientation' was, in fact, a diplomatic attempt to return to the pre-communist traditions of the discipline.

The 1959–1974 Period

The years after the death of Stalin and especially after Nicolae Ceauşescu took over the leadership of the party were a period of relative calm in Romanian politics and society. A climax was reached in 1968, when Romania opposed the Soviet invasion of Czechoslovakia, and started declining in 1971, the year when, after an official visit to China and North Korea (see Cătănuş 2005), Nicolae Ceauşescu launched the 'July theses' that put and end to this period of relative liberalization, clearly and irrevocably heading towards totalitarian national communism. Whether by chance or not, this was also the interval in which fieldwork done by Romanian ethnographers and folklorists – and, more and more often, by foreign ethnologists and anthropologists – developed the most, both in size and importance.

[8] I am referring to Dumitru Pop and Nicolae Bot, in Cluj; respectively to Eugen Todoran and Lucia Jucu-Atanasiu in Timişoara.

[9] Interview with Nicolae Bot, June 2005.

[10] The participants on these fieldtrips were also cautious concerning the importance and accuracy of their work (see interview with Maria Bocşe, February 2006).

Mihai Pop and the return to fieldwork

In its essential lines, the development of these research activities was headed and guided by Mihai Pop. As a head of the Department of Folklore at the University in Bucharest and as a director of the Academy's Institute of Folklore, he had the chance – and the skills – to select the best young students eager to work in this domain and even to re-orient those who had the slightest interest in popular culture by offering them good jobs, finding administrative solutions when they were in trouble, and by backing them when they had personal problems with the authorities. With his personal charisma and outstanding pedagogical skills, doubled by his 'unique diplomacy' (Karnoouh 1994: 22–5), Professor Pop created the flagship of Romanian folk studies during this period.

The first ethnological fieldwork he organized (in Peşteana – Jiu) took place in the summer of 1959, shortly after Mihai Pop had been appointed professor of folklore at the University in Bucharest;[11] the last (in Breb, in the region of Maramureş) took place in 1974, at the end of his period as director of the Institute of Folklore.[12] This period can be roughly divided into three stages. The first fieldwork took place during the early 1960s and did not seem to be very different from the former 'documentation trips', although eventually it had a further team dimension.[13] The second stage started around 1967–8, corresponding to the time when the young researchers hired by Mihai Pop at the Institute of Folklore in the early sixties became mature, good professionals and could perform research on their own. More than that, most of these researchers from the new wave were also able to initiate and coordinate the students' research.[14] The third stage began about 1970. Not only had the number of young researchers already grown substantially, but Mihai Pop also started to invite into the field dialectologists, linguists, psychologists, philosophers and artists, whose interests could in many ways be related to the study of village life. Under these circumstances, these teams increasingly resembled the complex research teams of Dimitrie Gusti.

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[11] Interview with Sanda Golopenția, May 2004; interview with Radu Răutu, October 2005.

[12] Interview with Paul Drogeanu, December 2005.

[13] See Sanda Golopenția's memories: 'And I went in the field with Mihai Pop many times. We started with Oltenia. So I went to Peşteana – Jiu and in this region we worked a lot on the fiddler's song. I remember that we did two or three research sessions. In one of them I was with Radu Răutu and Costel Cârstoiu, who were students by then, and were one year younger than I was, and we spent a good part of many summers listening to and transcribing fiddler songs.' (Interview with Golopenția, May 2004.)

[14] See interview with Paul Drogeanu, December 2005, and with Şerban Anghelescu, December 2005. It is also important to remember the fact that in these years fieldwork research became part of the regular curriculum for the students in folklore classes.

Compared with this Gustian approach, the Pop teams had a supplementary component: after 1968 they included Western ethnologists and anthropologists doing long-term fieldwork research in Romania. Their presence in the field and their participation in discussions about fieldwork also proved to be influential in an unexpected way: it forced native researchers (sometimes originally from the very village being studied) to *distance themselves from their familiar field*. Frequently, their foreign colleagues asked different questions or even observed things slightly differently. These differences had to be explained and understood.

An overall evaluation of this period should point to the fact that Mihai Pop's achievements meant re-linking the present with the valuable traditions from the pre-war period. Even if the idea of returning to Gusti's inter-war fieldwork was not officially stated until 1990, it is obvious that under Pop's leadership important steps were made in this direction. At the same time, his personal links and commitments with the Prague formalists, Greimas and Lévi-Strauss, caused him to encourage and help as much as possible the transition from 'Folklore' to 'Anthropology' in the Western sense of the word.

But how was all this possible at a grassroots level? How was this fieldwork feasible from an administrative point of view? Pop's strategies are telling in this respect.

It is beyond any doubt that the incontestable chief of Romanian folk studies knew how to build and maintain appropriate relationships with the authorities. If the initial sessions took place in the old locations of Gusti's teams (firstly, in Runc, Arcani and Dobrița), subsequent sessions took place where Mihai Pop's former students were appointed important political positions (in Vâlcea), and, in the end, in Maramureş, his homeland, a region that was so far away from the authorities in Bucharest, that any 'deviation' could remain confidential. These choices make clear that beyond the generic political approval 'from the centre', further local support was essential. And it was wise to nourish this support where one had the best 'connections', eventually close relatives.

Certainly, this was primarily administrative and financial support, meaning that the local authorities could offer extremely cheap journeys by making the best arrangements for sleeping and eating. Furthermore, the same 'local connections' could also help in finding local subsidies. Thus, for instance, the first fieldwork in Gorj was supported by a project concerning the folklore of the newly arrived workers of a neighbouring industrial platform.[15] Last but not least, with the tacit complicity of such friendly local

[15] Interview with Radu Răutu, October 2005.

authorities, research could be moved beyond the declared (and paid!) objectives toward the real interest of the researchers. Thus, in the above mentioned project, research took place in fact only in some villages that were considered interesting by their folkloric repertoire, instead of sticking to the local workers' new folklore. The archived material that exists at the Institute of Folklore stands as testimony to the intense research carried out on this campaign.

The fieldwork done in Maramureş in 1971 is even more eloquent in this respect. While the Institute's research team was camped at Sârbi, and the investigated themes were something like 'the local dialect, the traditional epics, nuptial and funeral ceremonies, the lyrical aspects',[16] Professor Pop took the young researcher Radu Răutu to a nearby village, Vadul Izei, to meet and work with one of the well-known sorceresses of the region. As a result of these interviews, not only were a series of document produced for the archive, but Radu Răutu also made an ethnographic film documenting in detail the magic practices. Being a taboo theme for the communist regime systematically 'fighting against superstitions', the whole research was made possible due to a smart idea of the head of the county's Health Department: he declared that every doctor had to know the magical practices in detail in order to control them. Consequently, the Health Department agreed to pay all the costs of the film, considered as a valuable scientific document of medical use.[17] Thus, until 1990, the film was presented only at a few medical symposiums.

Even if this aspect was less explicit, many of Mihai Pop's collaborators had thus learnt not only how to undertake observation and interviews, but also several useful political and organizational tricks.

But Pop's students were not the only ones to rediscover field research. The fever of fieldwork reached more and more people, many intellectuals getting involved in the documentation and publication of folk traditions. Their common approach was usually the reference model of the initial 'nation-building anthropology', thus reinforcing the traditional, pre-communist vision of folk culture and its collection. Meanwhile, the newly created activists of the communist propaganda, who were gravitating around the 'Houses of Creation', were doing their own fieldwork too. On one hand, their 'work agenda' consisted of imposing certain political agreed folk texts and undermining others; on the other hand, they usually just listened to people, rarely noting traditions – especially lyrical and epic texts.[18] Field-

[16] Interview with Silviu Angelescu, November 2004.
[17] Interview with Radu Răutu, October 2005.
[18] Interview with Ilie Moise, former activist at The House of Creation of Sibiu county, December 2005.

work and field collections during this period were thus a rather heterogeneous practice.

From practice to theory

With this growing number of fieldworkers and their extremely heterogeneous training, many grass-roots methodological questions arose: what does a researcher have to do in the field and why; what facts should s/he observe and under what circumstances; how should one approach people in the village; what materials should s/he choose and how to choose them; what instruments are necessary? All these issues and many others alike were of shared interest and could no longer be left to the researcher's inspiration. This is probably the reason for the several guide-like publications that followed one after another in this period.

The two leaders of Romanian anthropological research during these times, Mihai Pop and Ovidiu Bîrlea, felt thus obliged to answer this common need. If one looks beyond a sense of competition and even tension between the two, their respective volumes are, basically, convergent. Their main idea is that, contrary to common belief, fieldwork is not at all an easy game; most of all it is not a trip to the countryside or a quick writing down of information; it requires professional competence. This academic truism was directed, of course, at the number of untrained and self-proclaimed folklorists. Much more: in order to acquire such professional competence, one has to have an understanding of the whole profession. That is why such a 'manual' cannot restrict itself to a 'simple book of methodological recipes, a technical indicator of the way the researcher should act in the field' (Pop 1967: 95). On the contrary, a research guide has to offer, first of all, a detailed historical presentation of the main schools of study of popular culture, and to define folklore in some detail (Bîrlea's case), respectively the main characteristics of oral culture (Pop's case).

One can read between the lines the idea that fieldwork is actually the tip of the iceberg of a complicated science, and due to its difficulty, reserved to elites. Unfortunately, this permanent positioning of fieldwork at the end of a vast process with sophisticated cultural-historical prerequisites also had an involuntary and perverse effect: the authors almost never reached the 'end',[19] and, consequently, almost never came down to the precise details of fieldwork's investigation.

[19] One of the side effects of this option is primarily that few of the fieldwork's materials of communist Romania have been published. Placed at the end of the research chain and not at the beginning, fieldwork is about to become a purpose in itself, meant to confirm or reconfirm things that have already been written about.

Nevertheless, both guides manage to offer, in their own way, a sketch of fieldwork from a certain well-defined point of view. Mihai Pop's manual offered the perspective of a director of an institute coordinating large research teams. From this point of view he was mainly interested in how to 'place' the fieldwork by careful preliminary investigation, how to build and bring together the research team, how to provide the technical supplies (especially tape recorders), and how to design and use the questionnaires and the lists to be completed by the interviewees (Pop 1967: 114–16).

On the other hand, Bîrlea's book was useful more for the solitary researcher. His guide follows the way of going into the field step by step: it notes the difficulty of being accepted by the local community, recommends that direct relations with the interviewees should not be established, but to first get in touch with the local leaders (Bîrlea 1968: 50);[20] offers suggestions concerning the ways to win the local people's goodwill (*idem*: 55–9),[21] but without losing one's professional authority over them (*idem*: 218-19).[22] He ends with what he considers to be the main goal of any research: to be objective. This obsession with objectivity is actually shared by all the professionals of the period. Doing science means, according to them, being objective, no matter how many obstacles are in the way. Unfortunately, theoretical considerations of fieldwork ended here…

Anthropologists coming into the cold[23]

> My first field was only with Florica Lorinț at Runcu. And there I had spent my first 'field night' sleeping on a piece-of-wood-bed ... I almost couldn't sleep, I was feverish and only the next day I recovered. / It was a traumatising experience.../ Yes, but this was jut like that, as any young person who does fieldwork and tells him/herself:

[20] 'In order to remove from the very beginning any suspicions, the newly arrived folklorist does not have to get in touch directly with the informers but should be recommended to them by someone with authority in the village. Most eligible are the intellectual elites: first of all the school teachers and the directors of the clubs, the latter having as their duty to encourage these kinds of activities.'

[21] See: 'offered cigarettes, some candies for the children, win from the beginning the guests… an important problem is how to pay the informers. Commonsensically, the person has to be rewarded for the trouble, by cash or by a certain present, mostly something to drink… if you send a copy of the photo, the informer will be filled with joy, which will surpass other rewards and will gain a gratefulness for the researcher, not easy to forget.'

[22] See: 'it is not less true that being friends with the informers does not have to end up on familiarity terms, which can lead to under appreciate the work; the folklorist has to have the same attitude by which to impose on the informer'.

[23] Alluding to Steven Sampson's and David Kideckel's 1989 paper *Anthropologists going into the Cold: Research in the Age of Mutually Assured Destruction.*

> and now, what do I do on this bed? Ok, why was it interesting? Because it was a shock the fact that Florica Lorinț had a truly romantic-nationalistic vision of the Romanian village. She was telling me: you will see how it is; there are beautiful, long villages ... And when we discovered the villages of Runc and Dobrița I was really disappointed. It was all cement-dusty, we discovered pretty modern houses. That is, her story had nothing to do with the reality. And I think that made me sick and not the wooden bed. That was just a detail, but this was what shocked me, the discrepancy between what the ethnographers were recounting you and what you could actual see by yourself… Yes, this was actually the classical ethnographical myth but taken over in her very convincing discourse ...[24]

This snippet of memory by the Belgian ethnologist Marianne Mesnil gives us a very plastic image of the difficulties encountered by local and foreign anthropologists in the field.

The Western anthropologists were, in their turn, of at least two rather different categories. On one side, there were mainly European ethnologists, generally sharing what we actually call 'European ethnology', most of them invited or at least working directly with Professor Pop and his colleagues.[25] On the other side were the American anthropologists who followed John Cole soon after president's Nixon visit to Romania[26] in order to study a socialist society. Most of them, on both sides, had their fieldwork in Transylvania.

The 'ethnologists' were mainly interested in topics linked to local folk culture. Strangely enough, most of them started their fieldwork before learning the Romanian language.[27] This was important also because they required the presence of a translator, usually a young researcher from Pop's team. Even later on, when most of them were learning the Romanian language,[28]

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[24] Interview with Marianne Mesnil, October 2006.

[25] Jean Cuisenier, Marianne Mesnil, Micheline Lebarbier, Frank Alvares Fereira, Anne Guillermeau, Alain Bouras *et al.*

[26] Katherine Verdery, Gail Kligman, David Kideckel, Steven Sampson, Sam Beck *et al.*

[27] In this respect, Marianne Mesnil recalls the following personal story: 'My Ph.D. advisor who was an *africanist,* gave me a very good piece of advice: he told me that in order to enter a culture whose language I do not speak it is better to choose a subject that deals with objects. I conquered the language through objects. I still remember words that I have learnt in Maramureş which I am sure nobody knows anymore...' (Interview with Marianne Mesnil, October 2006).

[28] Anne Guillermeau recounts that, after a period of staying in Transylvania where she was studying Romanian dances, she travelled to Bucharest. When she got into a cab, the driver asked her where she was from. Anne hesitated knowing that foreigners were generally asked higher prices. In that moment the driver said: 'Well, dear miss, I can see for myself that you

they used to be accompanied on their fieldwork by some Romanian colleagues (Cuisenier 1994: 2000). Beyond the shared thematic interests, local and foreign researchers were thus building lasting relationships due to their continued presence as translators. This was probably the result of the fact that Romanian ethnologists not only translated what people were saying (this requiring a pre-translation into the literary language and also a good knowledge of several special and rare words in the foreign language), but also contextalized this information with their own interpretations or with abstracts from the Romanian bibliography concerning the problem in question, in order to make it more comprehensive for their foreign partners.[29] The lasting effect was not only a friendly relationship, but also better mutual knowledge and appreciation.

On the other hand, there were the researchers who came during official programmes, mainly the American anthropologists, who did not involve the Institute of Folklore. They were interested mainly in the current Romanian society writ large, their topics being thus much more politically laden. This also raised the problem of protecting their sources, speaking about everyday problems with a foreign citizen being much more risky than just telling him about a piece of traditional custom. In this respect, David Kideckel notes: 'The care for raw information of my informers who would have been identified, stopped me from discussing certain aspects of the local life' (Kideckel 2006: 21; see also Kligman 1998: 229).

The way of organizing their fieldwork was also different in some meaningful details. First, all of them already spoke Romanian pretty well when entering their field, so that they usually did not need Romanian translators. Even if they worked, at certain periods of their field research, with some local colleagues, this would not become a permanent and defining practice. On the other hand, all these anthropologists were committed to the classical Malinowskian type of lonely long-term fieldwork, rather than having short and repeated visits in search of rather limited information. It is interesting that the local communities did not lose sight of such differences, but interpreted them in their own way. Thus, in Maramureş, the peasants called Claude Karnoouh 'the Professor' and the researchers from the Institute of Folklore 'the tourists'.[30]

As a final and unintended effect of these differences, the 'ethnologists' remained not only closer to the Romanian folklorists and ethnogra-

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are from Transylvania. Nobody fooled me 'till now! I know the dialects from all over the country.'

[29] Interview with Radu Răutu, October 2005.

[30] Interview with Sanda Golopenția.

phers, but also more prized by them,[31] while the 'anthropologists' are still perceived as more distant people and professionals by their local colleagues,[32] being in return highly valued by post-communist elites.[33]

As their field reports and books slowly started to become accessible to some Romanian colleagues, they discovered a deeply different way of speaking about the field experience they were all sharing. What unites these texts, beyond chronology, method, theme or style, is the fact that they all refer to fieldwork as an essential starting point. Unlike their Romanian colleagues, all the Western anthropologists present the field as a *subject* in itself and not only as an important *place* of information.

The most representative text in this respect is, in my opinion, *The Wedding of the Dead* by Gail Kligman. Actually, a third of the book narrates the circumstances under which the researcher managed to observe and persuade people to speak, and to point at all of the contextual elements she uses later on in her interpretation. How it feels to be seen as a foreigner in a Romanian village, how a young woman-researcher is treated, what her place is within a peasant household once she is accepted there, what she can and what she cannot do in order not to harm the prestige of the family, how a foreign researcher can negotiate with the authorities, local or central, how s/he can escape the 'long shadows' of the *Securitate* (the political police), all these lived experiences sketch an extremely nuanced picture of what it means to do anthropological fieldwork in general, and what it was like to do fieldwork in communist Romania in particular. For most of the Romanian researchers this kind of approach was a great surprise. But for most of them it came (too) late – only after 1990, when this book and a few others, have also been published in Romania.

Toward the end of this period, a law concerning the regime of state secrets launched in 1973 stated that no Romanian citizen was allowed to accommodate a foreigner (except close relatives), and that any contact with a

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[31] It is probably not just by chance that in the *Dictionary of Romanian Ethnologists* European researchers are offered pretty consistent articles, while the only American anthropologist that is presented is Gail Kligman (who actually worked, several times, with people from the Institute of Folklore on a traditional ritual).

[32] Ilia Iliev suggested in a private conversation that this less friendly perception of the American researchers by the professional community of the Romanian folklorists might be a collateral effect of the fact that, speaking Romanian, they did not need any help from their Romanian colleagues, their mutual relations being thus more distant during the whole time.

[33] The recent 'Tismaneanu Report' on the crimes of communism mentions from its very beginning the importance of these anthropologists for changing the approach and understanding of socialism in Romania.

foreign citizen had to be reported within twenty-four hours. Fieldwork became more and more difficult for the Western researchers.[34]

The 1974–1989 Period

The last period of the communist regime in Romania was also the worst. Its way was paved by the 'July theses' in 1971 and confirmed five years later by the Congress of Political Education and Socialist Culture in June. It was a time of growing privations (rationing of food and fuel, repeated cutting off of electric power, water and heat, etc.), the country having to get rid, according to Ceauşescu, of all external debts and to become economically self-sustained; and it was a time of growing nationalist totalitarianism and cult of personality, and thus of growing international isolation.

This context had two contradictory effects on anthropological research. On one hand, the discipline was a victim of all these kinds of 'rationalizations' too: most of the departments were threatened with dissolution (and some of them were drastically affected), there was no more money for fieldtrips or for books and journals, etc. Thus, the institute in Bucharest amalgamated with the Central House of Creation, the Cluj branch became a small research department at the University of Cluj, the research group in Timişoara had to give up its traditional film-making activities, etc. And all of them had to give up their fieldwork and reorient their activity mainly toward the archives.

On the other hand, Ceauşescu's national communism put a very strong emphasis on folk studies supposed to back up the party's politics in this domain. Thus, the reborn institute in Bucharest not only could, but was obliged to, do extensive fieldwork in order to achieve a huge *national* project: *The Romanian Ethnographic Atlas*. But the kind of fieldwork it implied was rather different: the well-defined themes (household, occupations and traditional crafts, customs, the calendar's cycle and mythical representations) were covered by long lists of very technical items whose presence or absence had to be checked in a number of selected villages. This very 'technical' process oriented the discussions mainly on ethnographical topics and has drastically reduced the usual interviews and free conversations with the local peasants.

But folklorists and ethnographers were involved in the party's scenario much more then this: the researchers from the institute in Bucharest

[34] The following two stories recalled by David Kideckel from these times give a telling picture of the situation. The first is about the American anthropologist untroubled making microfilms of the *top secret* Five Years Plans of Braşov County he needed for his research (Kideckel 2006: 19). The other is about some of his interviewees who were punished, also by being fined, because they had talked to him (Kideckel 2006: 20–1).

were assimilated to the nomenklatura, being coordinated and controlled directly by the Propaganda Section of the Central Committee and having to coordinate and control in their turn the huge national festival 'Singing Romania' (see Mihăilescu in this volume). From a strictly organizational point of view this new status involved a series of 'privileges': better travel and housing conditions, open doors to local power, (limited) power of decision, and, last but not least, the possibility of embezzling some of these resources for other research interests. The price to pay was, to a large extent, the loss of the stable, close and friendly relationships with the local people that folk researchers used to have.

After Fieldwork

> After the time of field working has passed, the anthropologist returns home having different objects prepared to be interpreted and treated as concepts, with a series of technical terms and theoretical models which wait to be presented in a monographic text. In short, after the fieldwork there is writing time, the finality of the anthropologist's work being the fact that s/he delivers an elaborated text by which s/he communicates to a potential reader, generally a colleague (but not only), his/her experience in the company of the members of the society s/he has lived in (Kilani 1994: 45).

Everybody knows and agrees about this common procedure. But this was not necessarily the case with Romanian anthropologists during communism: in none of the interviews I analysed did the interviewees mention the fact that the interesting information, the suggestive stories and exciting problems they found or formulated during their fieldwork could or should have been the object of written texts. Thus, for Romanian anthropology, especially during communism, this final step of research, translating the experience into a public text, was rather an accident.

There are, I believe, several reasons for this paradoxical shift from the fervour of fieldwork to the silence that followed. The first concerns a *longue durée* tradition of the discipline, aiming at an exhaustive collection of national ethnographical and folklore documents. Going on to 'collect' and 'conserve' such documents during communism seems to have been a kind of moral commitment for many researchers, who thus also had the sentiment of fighting against the communist ravages of the 'true folklore'. In other words, this diffuse feeling of participation in a permanent process of saving and preserving the national patrimony in well-administered archives influenced the ways of writing, making personal articles less meaningful.

When it was nevertheless coming to writing, the Romanian canon – and especially that of the communist epoch – privileged a general presenta-

tion, with an omniscient tone over certain themes, genres, symbols, systematically neglecting the narrative experiences of fieldwork and all kinds of stories referring to the researcher's observations and attitudes concerning the lived realities of the village. In the very few cases when the researchers wanted to illustrate some interesting context issues, these were generally banished into endnotes.[35]

We have to add to this *longue durée* practice another of its defining elements: the total lack of fieldwork diaries, and thus of the routine of reflecting on one's work and problems while doing fieldwork. This shared lack of interest may itself be explained by at least two types of reason. On one hand we have to mention the positivistic obsession with objectivity; the researcher's personal reactions, and implicitly the actual experiences of fieldwork, being considered, in this perspective, much too 'subjective' to be of any relevance. On the other hand, Romanian anthropology was about unity, it was a nation-building science, which had to help build the nation as a united historical subject. In this context, detailed presentations of individual fieldwork experience could have functioned as a truly disturbing element to this unity. Instead, collecting materials and putting them into archives in order to fill up a pigeon-hole within the great unity of the cultural folkloric reality was really the proper and preferable way to proceed.

The complement of all these rather structural reasons is a very strong contextual reason peculiar to communism: writing about fieldwork experiences means (also) to write about the everyday life of the people you were living and talking with – or such genuine reports about 'real life' were not what the party was expecting from folklorists and ethnographers![36] Not writing about the fieldwork experience was thus also in its way, a self-censorship.

Broadening the context, if we except the few monumental works that fitted into the general frame of national communism, publishing was extremely difficult in general, and practically impossible in occidental journals. In time, publishing became less and less a main concern, and professional writing was more and more out-dated. A strange and final consequence of all

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[35] We can find extremely interesting notes to rather dull texts of the Romanian bibliography from the communist epoch, first of all in Bîrlea 1966: 333–60; Bîrlea 1968: 221–46; Crețu 1980. Regarding the latter of the three volumes, the director in those times, writer Ion Marin Almăjan, remembers that the direct references to fieldwork and to what people said were removed by censorship. Finally, the publishing house decided to publish them as endnotes.

[36] A special case is that of the ethnographic films that were live recordings of different folkloric festivals (see for example the 43 ethnographical films from the Archive of Folklore in Timişoara), having a visual discourse that communicated a certain particular message, while the verbal one was avoiding any kind of references to the particular context of the film, concentrating only upon general issues.

this was the fact that academic prestige was not necessarily built on writing, and even less so on research papers!

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At the end of this historical overview, one may conclude, I believe, that fieldwork was a permanent and central *practice* of Romanian ethnography and folklore studies even in the communist epoch. In order to continue with this practice, many researchers accepted all kinds of compromises with the regime. For the younger ones, going into villages on fieldwork was first of all a kind of rite of passage. To some extent, it was also a way of escaping daily life in communist towns and institutions. But more important, according to a long-lasting trend of the discipline in Romania, these fieldworks were meant mainly to complete the national archives. Managing to do this, regardless of the difficulties, seems to have been a moral and professional duty for most of the researchers, many of them having also the strong belief that by doing so they are fighting communism too.

Unfortunately, archives were to a large extent the end point of the discipline: once completed, they just closed their doors.

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Chapter 3
De Gusti*bus non disputandum*[1]. . . Romanian Folk Studies in the Fifties

Constantin Eretescu

Folk studies were collateral victims of the communist system installed in Romania with the 'help' of the red army in the second part of the 1940s. The field was not intended for suppression, as was the case with sociology for instance. On the contrary, experience showed that folk culture could be used effectively for propaganda purposes by the new authorities. Nevertheless, it needed a decisive new orientation.[2] This had to be achieved by a parallel attack on former cultural institutions, publications and personalities.

Institutional Cleansing

During the inter-war period, an increasing number of centres for collecting and studying national popular culture gained scholars' attention. Their publications were often quoted in the written media of the time and thus they could be widely read. The Folklore Archive of the Romanian Academy in Cluj was probably the most important among these centres. It was founded on Ion Muslea's initiative in 1930. At the time a young researcher at the Transylvanian Ethnographic Museum, freshly graduated from the University of Cluj, Ion Muslea had also followed specialized courses at the College de France. Along the years, Muslea had distributed no less than twenty questionnaires on a variety of topics such as customs related to the human life

[1] Dimitrie Gusti, the founder and leader of the Sociological School from Bucharest, became *persona non grata* after the communist takeover. A sad joke was thus shared by his former co-workers, saying that *de* Gusti*bus non disputandum*!

[2] There are few written testimonies left by those who passed through that period, and the scholars of folk culture are no exception. The information related to the first years that followed the communist takeover has just begun to be collected and studied. The recently published volume *Other Contributions to Romanian Ethnology*, Universal Dalsi, Bucharest, 2004, by Iordan Datcu is a first overview in this respect. The work includes a well-documented chapter on 'Ethnography and Folklore between 1948 and 1958' to which we shall often refer.

cycle (birth, baptism, childhood, wedding and death), mythology (beliefs regarding water, earth, sky), juridical customs or traditional institutions, beliefs, foresights and adumbrations. Thereafter he gathered 1,064 manuscripts consisting of 416 answers from Transylvania, 232 answers from Moldavia and 227 answers from Muntenia and Oltenia. By the time the Folklore Archive of the Romanian Academy was dissolved, the number of the manuscripts had reached 1,221 (cf. Muşlea 2003a: 60).

The Folklore Archive of the Romanian Composers' Association and The Phonogram Archive of the Ministry of Cults and Arts shared a similar fate. The former, founded by Constantin Brailoiu, affiliated from its very beginning to the investigations carried out by the Romanian Social Institute lead by Dimitrie Gusti. It participated therefore in the big monographic campaigns undertaken in almost all traditional areas of the country. The data collected in these studies were gathered in an archive which comprised 30,000 musical pieces in 1940. This archive appeared to be the most representative depositary of national popular culture. Brailoiu's departure to Berne in 1943 blocked the activity of the institution. Fortunately, the collected data were transferred to the corpus of the new folklore institute. Nevertheless the awkward methods of depositing the wax cylinders that the data were recorded on caused the destruction of most of it as well as irretrievable losses. The Phonogram Archive of the Ministry of Cults and Arts, founded by George Breazu upon the model of the Phonogram Archive lead by Erich M. von Hornbostel, had a similar destiny. During more than two decades of the archive's existence (1927–48), the folklorists gathered around G. Breazu had collected around 10,000 melodies. Breazu's contribution towards the understanding of the folkloric phenomenon is still underacknowledged. He is counted among the fathers of the Romanian ethnomusicology. He is one of those who set up the basics of scientifically collecting musical folklore. The institution he founded died once the authorities stopped granting the subsidy necessary for carrying out research and for safeguarding the collection.

Besides the institutions mentioned above, of great importance are the scientific circles which brought together researchers of popular culture. These circles were organized by criteria of either region or of scientific interests. Hereby Romulus Vuia founded The Circle of Ethnographic Studies in Cluj, in 1939. The same year and in the same place, Gh. Pavelescu established The Circle of Monographic Studies. Neither of the two lasted long. In Bucharest, the folklorists founded The Circle for Folkloric Studies which lasted for two years (1945–7).

The study of the popular cultural phenomenon was an essential and constant feature of the monographic research carried out under the aegis of

the Sociological School from Bucharest, lead by Dimitrie Gusti. The most important folklorists of the time were involved in the monographic studies conducted in villages, illustrating the social structure of Romania of the time (the first decades of the twentieth century). Among others, E. Bernea, C. Brăiloiu, I. Chelcea, Il. Cocişiu, Şt. Cristescu, Gh. Focşa, A. Golopenţia, M. Pop and P. Ştefănucă took part in these campaigns. All this prestigious activity came to an end shortly after the installation of the communist regime.

In some cases the buildings which housed the institutions of popular culture shared the same fate. Established in 1906, The Museum of Ethnography, National Art, Decorative and Industrial Art which became The Museum of Ethnography and National Art under Alexandru Tzigara Samurcas' direction, and later on, The Museum of National Art, was evicted from the building which had been housing it for decades. Instead of it, a museum of the history of the Communist Party was established on this location. Barbu Slatineanu, author of some fundamental studies about ceramics, organizer of the collection of decorative art and popular ceramics, saw his house share the same fate.

The majority of the scientific publications in the field disappeared shortly after the communist takeover, although most of them had succeeded in surviving the difficult wartime. I. Muslea, the author of the bibliography of the Romanian folklore between 1930 and 1955, noted that there were 114 publications containing works on folklore topics in the period 1944–50 (Muslea 2003b: 196–7, 268). The following years (1951–5), the number of this kind of publication decreased and reached just 36. Among them, *Studies and Researches in Literary History and Folklore* was the only one to partly address folkloric research.

Most of the publications began to disappear at the very end of, or immediately after, the war. Publication of *Romanian Sociology* ceased in 1944, only one year after the disappearance of The Archive for Science and Social Reform. Similarly, *The Review of the Banat-Crisana Social Institute*, the last publication of the Sociological School of Bucharest, came to an end in 1946. *Dacoromania* and *Oltenia Archives* stopped coming out in 1944. The last issue of *The Yearbook Folklore Archive of the Romanian Academy* was released in 1945. Publications with an old tradition, such as *Transylvania*, which had been continually published since 1868, ceased existence in 1946. The case of the *Romanian Telegraph*, first released in 1853, was the same. Both addressed a wider range of subjects. Popular culture was only one of the topics in which they were interested. The *Folkloric Researches*, whose small volume came out in 1947, was the last survivor in a long series of disappearing reviews. The magazine brought together works of the Circle of

Folk Studies where specialists as G. Breazu, I. C. Chitimia, I. A. Candrea, Gh. Vrabie, R. Vulcanescu and others were gathered. The magazine aimed at conceiving some reference works such as an exhaustive bibliography, a corpus and a Romanian folkloric atlas.

Nevertheless, the effects of the installation of the new regime were felt most dramatically by the scholars themselves. A significant number of folklorists were forced to completely abandon their careers. Some of them would never re-enter the profession. Others would publish articles in the scientific press only after many years or, indeed, had their works published posthumously. The brilliant comparatist Petre Caraman was completely expelled from the academic field and he henceforth earned his living working as a porter at the town's railway station. He was forbidden to publish a single book for the rest of his life. The case of Gh. Pavelescu was the same. In 1949 he was expelled from the party and the most severe accusations were made against him: anti-Soviet, fascist, enemy of the people. On the 30 May 1950 he was dismissed from the position of director of the Ethnographic Museum in Cluj. Furthermore, in 1950, his lecturer position was suppressed on 'budgetary economical' grounds. From 1952, he lived in 'intellectual exile'(Datcu 2004: 149–50). In some other cases, the regime sent scholars to prison. D. Caracostea was arrested for having occupied official positions in the 'old regime', and sent to one of the extermination prisons for years without even appearing before a judge. I. Diaconu, O. Papadima, E. Bernea, Tr. Herseni, H. Brauner, and D. Şandru shared the same fate. Anton Golopenția and Mircea Vulcănescu, representative members of the Sociological School from Bucharest died in prison under dramatic circumstances. Barbu Slatineanu had the same destiny, as did the Basarabean folklorist Petre Stefanuca. The latter was sentenced to death by an exceptional Soviet court and later died in a concentration camp.

Many more could only enter the profession either later or conditionally. This was the case with Vasile Băncilă, L. Blaga, N. I. Dumitraşcu, T. Găluşcă, Tr. Gherman, I. Muşlea, T. Papahagi, and H. H. Stahl. Others chose to leave the country. Mircea Eliade, having published a great number of Romanian folkloric studies in Romania before the war, became historian of religions. He settled in the USA. C. Brailoiu lived in Switzerland and France, O. Buhociu and D. C.Amzar lived in Germany whilst P. Iroaie lived in Italy. Nevertheless not all acts of self-exile found a happy end. Ion Ionica, author of an excellent monographic study dedicated to an agrarian ceremony ('the Coronet') disappeared (Ionica 1943). He was most probably killed by the guides who were there to help him cross the frontier between Austria and Switzerland. Apparently, the guides were irresistibly attracted by the heavy coffer that Ion Ionica wouldn't part with. The assumption that the box was

filled with gold determined its owner's fate. They would have realized their mistake too late, finding only manuscripts and folklore books in the box.

Most of the Romanians settled abroad carried on their studies in the field for which they had been trained in their own country. Thereafter, C. Brailoiu, one of the fathers of ethnomusicology, organized the archives of popular music in Geneva (*Les Archives internationales de musique populaire*, 1944). O. Buhociu conceived abroad his most important work, *Die rumänische Volkskultur und ihre Mythologie* (Buhociu 1974). M. Eliade wrote outside Romania *De Zamolxes à Gengis Khan*, one of his fundamental studies addressing the relation between folklore and mythology inside Romanian culture.

The physical elimination of inconvenient culture personalities was accompanied by the censorship of their works. The volume *Gândirea interzisă. Scrieri cenzurate în România 1945–1989* (*Forbidden Thinking. Banned Writings in Romania during 1945–1989*) published in 2000, contains long lists of books that were meant to be removed from the libraries and banned (Caravia 2000). We recall here only a few: V. Alecsandri, *Poezii populare ale românilor* (*Romanians' Popular Poems*), Alecu Russo, *Cântarea României* (*Praising Romania*), M. Olinescu, *Mitologie românească* (*Romanian Mythology*), and works of I. A. Candrea, C. Brăiloiu, G. Dem Teodorescu, L. Blaga, S. Mehedinți, H. H. Stahl, T. Bălăşel, E. Bernea, I. G. Bibicescu etc.

It was not only individuals and their works that were subject to purge, but also whole fields of studies. In the field of oral culture studies, topics such as magic (sorcery), exorcism, carols, funeral rites and religious works were banned. In the Introduction to the *Bibliografia folclorului românesc pe anii 1951–1955* (*Bibliography of Romanian Folklore in the years 1951–1955*), Muslea noted the sudden changes that had occurred:

> I dropped out the chapters 'Bibliography' (no file), 'Customs and beliefs' (one file for holidays and one file for juridical customs), 'Carols'(only one text), 'Mythology and religious folklore' (two to three files), 'Popular medicine and exorcisms' (no file – the same as in case of 'Ethnobotanics', 'The Magic' and 'Superstitions'). The absence of these chapters is illustrative for the new trends in our folk studies (Muşlea 2003b: 248).

A few scholars were estranged from scientific activity by means of the disappearance of the subjects of their studies. This was the case, among others, with Stefania Cristescu Golopentia who authored a volume awarded

by The Romanian Academy in 1941. Her field of studies was the practice of exorcisms.[3]

Consequently, for a long period of time, a great number of scholars devoted themselves either to historical studies or to studies that evoked personalities, much more 'neutral' politically.

New Folklore Comes to Romania

The institutions, the individuals and research activities that survived this purging process were required to follow the new orientations of socialism. In the field of folklore, as in all other domains of social life, the specialists were instructed to follow the principles of materialism and the Soviet model. With this end in view, articles that had initially come out in the Soviet Union, were translated and published: for example, A. M. Astahova's 'The significance of Stalin's works on linguistics in the development of the knowledge about poetical popular creation' (Astahova 1952: 145–59), Mitu Grossu's 'The Soviet conception of folklore' (Grossu 1953: 247–8), A. Z(acordonet)'s 'The Soviet criticism of the Romanian popular poetry' etc. (1955: 118–20). The magazines accommodated works such as Valentin Damian's 'Popular creation as a lively mirror of the nation's fight for a better life' (1954: 3–9), Faragó József's 'Sztálin a hazai népköltészetben' (Stalin in the popular poetry of the country) (1950: 6), Ileana Vrancea's, 'Doinele' (1950: 246–62), C. Bărbulescu's 'The creation of new popular songs' (after the 23 August 1944) (1952: 113–220); Paula Carp's 'How the folk conceives new songs' (1951: 2); Ion Socol's 'What if the Party weren't' (popular poem) (1955: 1); Ioanichie Olteanu's 'About a young folklorist and about our new popular poetry', etc. (1950: 4). There are few acolytes of the former culture among the promoters of the new trend. One of them was C. I. Gulian, who wrote *The Meaning of Life in Romanian Folklor*e (Gulian 1957). The content of this work was made up of chapters such as 'About the social content of our folklore', 'Masses' attitude towards misery and suffering', 'The rebellion against oppression and against exploitation', 'Hate between the classes', 'Hate against feudal and bourgeois land lordly state' etc. Eusebiu Camilar and G. Breazu were two others acolytes of the former culture who reconverted. The former published 'The Bistrita log driver's ballad' (Camilar 1951: 4). Apparently Eusebiu Camilar's own creation, the ballad, was presented by its author as having been collected in the field. Breazu applied himself to discovering songs dedicated to the party. He published a few articles on the topic.

[3] For publication list, see Golopentia 2002: 23–5.

The existence and the circulation of the new workmen and peasant folklore was a major stake in the political regime. This kind of folklore was meant to acknowledge the popularity, among the masses, of the political actions taken by the authorities. They made the assumption that folklore was a lively phenomenon in Romania at the time. It was subsequently essential for the great political events in the country to be put into poetical expression by those who handled the rhetoric of the genre. In the cultural leaders' opinion, song was the most sensitive genre, and the most capable of addressing the changes that occurred in society. It was thereafter in the genre of song that new oral productions should develop. M. Pop, an influential researcher who had long ago entered the profession, acknowledged that 'the renewal [of the folklore] is one of its fundamental principles' (Pop 1959: 57–66). Nevertheless he inserted some nuances and stated that changes did not occur instantaneously:

> In the process of renewal, the old is not forgotten, it does not change all of a sudden. And the new is shaped and imposed gradually. Not only during a longer period of time, but also at present, the tendency of keeping and the tendency of renewing the tradition operate simultaneously. Therefore the contradictions between them are inherent.

And furthermore:

> Changes in folkloric life address the function, the content, the subject and the structure of popular creations. Nevertheless they do not all emerge with the same intensity in all the fields and in all the genres. They do not have the same significance in all our folklore areas and in all the places of a region, as we have found (*idem*).

The release of these songs praising the new regime was belated. Thereafter the need to encourage such creations became imperative. Cultural activists, peasants eager to be poetically acknowledged and even local poets were meant to fulfil this need. On the other hand, the new songs voiced different attitudes, even antithetical to those expected by the authorities. Incidentally, some arose during the very communist period, hidden within 'revolutionary songs'. The majority of these songs have only recently been retrieved as an expression of anti-communism resistance. For example:

> The flying skylark sings
> When it hears the tractor
> The cuckoo sings in the garden
> When it hears the harvester
> Even the hoopoe sings
> When it hears the thresher,
> The fields ripened
> The barns got filled

The vineyard ripened
The barrels to be filled.
In the tree the crow sings again
Calling a cold winter to come.
I do nor care, let it come
I stay inside my household.[4]

E. Cernea and V. D. Nicolaescu ,the authors of the article 'About the first collection of new songs from Dobrogea' (Cernea and Nicolescu 1959: 49–74), tried to explain the enthusiasm felt by the author for this new song: 'The whole song creates the image of abundance, of a general happiness shared with the birds I the sky. Nobody has a care in the world. The poor people who usually fear cold winter have no worries about tomorrow. The collective household [the collective farm] guarantees its members a prodigal living' (*Op. cit.*, p. 65).

In order to make these new poetic products fit into the category of folklore, a new theory was needed. This should reconsider the relationship between the individual and the collectivity. Is the song the result of the creative effort of a single person, more sensitive or more gifted? Is this song thereafter circulated in order to be legitimated by a collectivity whose main role is to be a good receiver and eventually a diffuser? Is the ear of the collectivity the organ which catches up with the song produced by an author? Is the community's participation in the process of dissemination really needed? The disentanglement of this relationship was necessary mainly because the new folklore was clearly a product of certain individuals. Secondly, the members of the social group had no role in the circulation, the improvement, or the completion of the artistic object. V. Adascalitei was the one who assumed responsibility for redefining this relationship, in the article 'About the act of folkloric production' (1959: 25–44). The study was not programmatically conceived in order to validate these new literary productions. Nevertheless it offered the theoretical bases needed to fulfil this aim.

The contribution of the individual and of the collectivity in the act of creation was the topic of some remarks made at the time by predecessors of the young author. I. Diaconu stated that 'the literary coagulation is the product of a single bright individual, of a character gifted with creative power' (cf. Diaconu 1935: 7–23; Adăscăliței 1959: 26). Al. Dima voiced a different, but not completely opposite point of view: 'Popular art can be consequently understood as the art of the community which occurs by means of individuals' (Dima 1939: 133). 'The individuals played only the secondary and accidental role of the organ of expression, the lonely top through which a

[4] Text included in Munteanu; cf. Cernea and Nicolescu 1959: 65.

collective feeling is relieved' (*idem*: 121). They ought to place the new creation between these two different approaches. Without deliberation, the author excluded the human groups that didn't share the point of view of the lower and oppressed class from the category 'folk'. Consequently any dissident literary work that contravened the official point of view could not be asserted as folklore even if it fulfilled the requirements of the oral tradition. Such an art

> cannot be considered folkloric because the folklore is an artistic genre belonging to the social categories and classes which have a progressive role in the society development. The social classes and categories that do not belong to the popular masses, to the folk – seen as builder of the history – have a distinct culture because they have a distinct ideology (Adăscăliței 1959: 36).

The collective character is one of the main features of folklore. The circulation of the text and its constant conformation to the new contexts engender new variants and facilitate new creations. Nevertheless the dissemination of such new songs by means of the stage, the radio and later on of the television, was not proof of the folkloric character. When circulating, these songs were performed by their composers without being appropriated by the human collectivity. The Soviet V. I. Cicerov further remarked that a text could be considered folkloric even if its production was frankly individual and the collectivity did not introduce it in a circulation process and did not contribute to its transformation (Adăscăliței 1959: 39).

We can conclude that the discovery of new songs which enthusiastically articulated the new life-style was at stake in the epoch. And the 'folklorists' should identify these new Romanian folkloric creations and even fabricate them … Therefore the process of decorating the new reality with red flags started.

The New Folklore and its New Institutions

In order to ensure control over the field, new institutions were founded under the direction of government authorities. Established on the 6 April 1949, The Folklore Institute was the first institution for researching popular culture after the communist takeover. Today it functions under the name of 'C. Brailoiu' Institute for Ethnography and Folklore. Harry Brauner, Brailoiu's ex-collaborator, was its first general manager. He configured the profile of the institution, hired the employees and established the objectives to be pursued. He took over and brilliantly organized The Folklore Archive of the Romanian Composers Society and The Phonogram Archive of the Ministry of Cults and Arts. Besides conducting the study of traditional cultural phenomena, he enlarged the collection of studies by means of recorded docu-

ments, videos, photo collections and manuscripts. A department of the folklore institute in Bucharest, initially assigned to collect Hungarian folklore, was established in Cluj. A Romanian musical folklorist (Ion R. Nicola) was hired here. A few years later, in 1957, I. Muşlea, Valer Butură and D. Pop joined the institute. Consequently the collections of The Folklore Archive of the Romanian Academy once again became a part of the new department's stock of folkloric documents. The Iasi branch of the Romanian Academy established a folklore department as well. It aimed at collecting, salvaging and studying the popular phenomenon in Moldova. Established shortly afterward, The Institute for History and Literary Theory had a folklore department lead by I. C. Chitimia. Specialists such as O. Papadima, Gh. Vrabie or Al. Bistrițeanu worked here. The Universities from Bucharest, from Cluj, from Iasi and from Timisoara re-inserted into their curricula, in new forms, the study of elementary features of national popular culture. In this way the relations with the inter-war folkloric studies were partially and somehow resumed within the academic field.

On the other hand, the new Central House of Popular Creation was meant to become the spokesman of the new ideology. Established in 1953 in the Soviet fashion of the 'N. C. Krupskaia' Union House of Popular Creation, it was charged with organizing amateur artistic movement and with giving an impetus to new folkloric creation. With branches in each county of the country as well as dance groups, choruses and rhapsodists in each village, in time it became a powerful institution. In order to accomplish its mission, it was equipped with a publishing house that delivered for free hundreds of titles. Thousands of copies of these books were published over almost four decades. The generosity of this process engendered, after some years of amateur artists' competitions, the initiation of a huge national contest known as 'Cantarea Romanie' (Singing Romania).

In 1952 the Institute for History and Literary Theory started publishing the magazine *Studies and Researches in Literary History and Folklore*. It accommodated studies of popular literature in its pages. It was the first publication to contain folk studies after the dissolution of the magazines in the previous era. Nevertheless *The Folklore Magazine* (originally in 1963 called *The Ethnography and Folklore Magazine*) was the publication where most scholars of popular culture gathered. Its first issue came out in 1956. The composer Sabin V. Drăgoi, the manager of the folklore institute at the time, published the 'Foreword' where he confirmed the collaboration of 'all the folklore scholars who have something to say of general interest and which could help our work develop' (Drăgoi 1956: 8).

This relatively well-balanced state of affairs came to an end only three years after the repression of the revolt in Hungary. The publication of Ileana

Vrancea's article 'About some studies in literary folklore' raised the alarm signal in the field of folk studies (Vrancea 1959: 98–111). The author (who had no connection with folklore studies) voiced for the first time a 'cultural revolution' (*idem*: 98) which was taking place and which thereby engendered the fact that 'the development of folk studies could not be done without the dissolution of the ballast consisting in decayed ideas, reactionary in the field of folk studies … Because they brought advantage to the interests of the bourgeois and landlords classes' (*idem*: 102). The article contained a long list of scholars who were not sensitive enough of the battle between the classes in the field of folklore.

After completing his ethnographic studies in Moscow, I. Vlăduțiu threw himself into the fight against non-Marxist ethnography and chose the same 'Lupta de clasa' (Fight between the classes) as a stage alibi (Vlăduțiu 1960). The author started his debate with the assumption that 'the ethnographic research of social reality has deep class content'. He could therefore scientifically disqualify all the ethnologists who didn't approach all spiritual performances from this angle. Furthermore, he accused them of 'reactionary attitude, mysticism, nationalism, idealism, racism, and archaism'.

The effect on the folklorists of the emergence of these articles was quick and dramatic. The first issue of *The Folklore Magazine* that came out after the publishing of the critical work had a content that differed from all earlier issues. Half of the content of the magazine approached the new folklore in different contexts, the amateur orchestras, the social aspects of the oral literature, all from the perspective of Soviet scholars' knowledge. Things would follow this direction in the future issues. This way a new domain would be set up artificially. It addressed individual productions which had no common feature with popular literature. Only towards the middle of the next decade could one note a different approach. This happened when Romania temporarily opened up towards the Western world.

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It is difficult to count the prejudices brought to Romanian global culture and particularly to the field of popular culture by the politics of the communist regime during this decade. Unlike Western Europe, where folkloric culture had practically vanished, Romania still had a productive oral culture. Suppressing the right to testify to the existence of, and the right to study, large categories of the phenomenon, as well as punishing those who carried on performing customs forbidden by the regime (e.g., carols, magic rites, popular medicine, exorcisms) engendered the evanescence or drastic alteration of some archaic practices and occupations before they had become the topic of

firm studies. The disappearance of a custom from the field of spiritual popular practices is irretrievable. In this case, forgetting means dying.

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Chapter 4
A New Festival for the New Man: The Socialist Market of Folk Experts during the 'Singing Romania' National Festival

Vintilă Mihăilescu

Staging national culture is part of any nation building and of the national being in general. Staging peasant 'traditional' culture as the very national identity is not an uncommon strategy – and the case of Romania was typical. Communism has also used such festivals and contests in order to celebrate its own achievements and values. In some national–communist versions, former traditional culture and the present revolutionary culture are mixed together in order to produce and stage a national genealogy (and legitimacy) of the communist 'new man'. After the few years of 'opening' at the end of the 1960s, this was the case with the 'Singing Romania' National Festival (*Cîntarea României*, CR in the following pages), designed and imposed by Ceauşescu's own brand of national communism. Belonging to a large family of such national(istic) festivities, CR was nevertheless rather special because of its all-embracing scale and its lasting social impact. A central field of concern, folk culture (and its experts) never recovered after this vast social experiment.

1. What was CR?

Strangely enough, seventeen years after the fall of communism, there are still no real testimonies and almost no research on this phenomenon apart from some private nostalgia and a rather common public revulsion. 'This subject is taboo. Everybody avoided [speaking about the CR]…Their reasons for keeping silent on this subject can be different. Either they were so strongly involved in right and wrong things that they are incapable of discerning it yet, or they are tired. Actually, they cannot be objective them-

selves anymore. And one can understand this' (A.).[1] In fact, what we have is rather a kind of

> folklore about this folk festival. We rely more on vast and truthfully anecdotal evidence of CR. Because people have been living for a while in this general climate, whenever they are asked to speak about it, they do not recall festivals in the centre of the town nor the shooting from a helicopter. They would rather recall from their highly subjective memory small events such as an accidental dismissal of an unknown teacher. He was rejected from university because he had witnessed an immorality trial, brought on the basis of his having been member of a CR jury. Another example is the memorable story of a small ethnic minority in Caraşova. It ought to have participated at CR but nobody in Caraşova could sing. Thereafter the priest was disguised as a pop singer, stepped on the stage and was awarded. Last but not least, there is the story of a lady, an ex-organizer of CR in the county of Caraş Severin who told that even Nicolae Ceauşescu had burst into tears when a folk group performed a funerary ceremony on the stage. One can hardly believe this story when funerary topics were rarely performed on a stage… (Hedeşan 2006).

I myself had the same experience when trying to speak about CR with some former party activists. The first thing one of them told me was about an important party leader who decided to have a feminine team for the *Căluş* (a typically masculine ritual dance), because 'in socialism men and women are equal'. 'But it didn't work, so that the girls danced only one summer… It was totally stupid, but, you know, there were also smart guys in the CR!' (P.). On the contrary, most of the 'intellectuals' I tried to address generally cursed the CR and the people who were involved. Finally, one year ago, when organizing a round table on this issue with fellow ethnologists, each and every one of them had another, quite personal, interpretation of what, in fact, CR was. The most salient ones may be summarized as follows:

- it was a diversion, a kind of *pane et circensem*, growing more and more triumphal with the growing economic decline of the society;
- it was a strategy to control people's spare time, i.e. 'free' time, mainly its most powerful expression, the time devoted to creation;

[1] Interview realized by Otilia Hedeşan with A. in 2006. Wherever a capital is used instead of a name, this means that the interviewee wanted to remain anonymous.

- it was a means of obtaining the goodwill of the peasantry, still the largest class of Romania and the most affected by communism; it was not by chance that the peasant was, in fact, the main winner of this contest;
- it was a genuine response to the Brezhnev doctrine, opposing national particularism – and thus independence – to the soviet homogenization;
- it was the expression of a growing and typical Romanian ultra-nationalism, fuelled to a large extent by the fear of the so-called Hungarian irredentism;
- it was a form of ethnic cleansing in as far as it selected and staged the 'pure' Romanian values and artefacts, as well as the 'pure' cultural expressions of the so-called 'cohabiting minorities'. It was thus fixing and controlling the cultural canon of each ethnic population, with Romanians at the top of the hierarchy;
- it was just the climax of a natural development of communism, that took place in all communist countries; CR is not different by its nature, but just by its size;
- besides these differences, almost everybody agrees that CR was above all a means of building the *new man*. Differences reappear when it comes to explain what was meant, in fact, by the *new man*…

No wonder then that most of these people opt for a kind of anecdotal history and for an impressionistic interpretation of the CR social phenomenon: 'Nobody was giving a definition of anything! They didn't preach clear concepts; they were mainly using intuition and actions derived from these intuitions… I reaffirm, nobody was able to utter a coherent discourse about the objectives and principles they should follow during the CR…' (Rădulescu 2006). The opposite opinion, perceiving CR as a careful social construction, is much less shared. Rodica Zane, for instance, defends the idea that 'CR cannot be the work of the activists' "intuitions". I believe that this phenomenon is a complex construction that can be analysed alongside other constructions characteristic of the Romanian communism. One of them is the well-known politics of demographic control' (Zane 2006).

Finally, it seems that – as for communism in general – the Romanian social memory is still wavering between love and hate – with funny stories as a way of making both sides meet.

It is hard, in this context, to produce a comprehensive interpretation of the whole phenomenon. The present text will thus restrict itself to a preliminary systematization of facts and ideas.

Historical Antecedents

To some extent and in some respects, this brand new cultural event was, in fact, an old wine in new bottles: present national(istic) festivities are always rooted in some past. Similar huge festivities were organized, for instance, during the times of Carol II before World War II, as part of a 'political religion' quite common to totalitarian regimes (Bănică 2006). Nor is the practice of selecting, orienting and promoting folk culture by some leading elites unprecedented.

> In the Twenties, the 'Arts and Crafts' movement was prizing the decorative arts and the traditional kind of objects, which were deeply admired by plenty of bourgeois intellectuals. The sometimes very direct intervention of people and cultural institutions … imposed over the peasant production the new constraints of the national cultural market. The more explicit this 'national' character is in these objects, the larger its market demand … The transformations of the rural mentalities resulting from this process were in many cases as fundamental as those determined some decades later by the contests and folk representations of the Ceauşescu era (Popescu 1995: 404–5).

Nevertheless, the former transformations are considered by many Romanian intellectuals as 'good' and legitimate interventions: 'The Carolingian version, I mean what Carol II did before war and all this stuff was very good, it was in the line of Gusti's *Singing Romania*', an important ethnologist claims in a recent interview.[2]

Recycling some of these historical features for the sake of communism, the new festivals had different aims and used different means. In fact, before CR started in 1976, some other national festivals and contests had been organized, but in much more modest forms, more or less similar to what was going on in all the communist countries.

With communism, the staging of culture involves a competitive dimension, with a strong Stakhanovism flavour: at the local and/or national level, people have to compete for more and better artistic works. Starting with the 1950s, different forms of contests were aimed at strengthening the different branches of *cultural production* too. As paraphrased by a local party leader, 'in Mehedinți county, as everywhere else in the country, hard work interweave with song and dance' (Matei 1976: 37).

This interweaving of production and culture gave birth to a kind of *cultural Stakhanovism*, turned, to some extent, into a specific socialist tradition. Thus, when the CR festival started in the county of Bacău, the local

[2] Interview realized by Otilia Hedeşan with D. in 2006.

party leaders emphasized a socialist continuity, claiming that 'we are starting from former achievements turned to tradition, such as the *Festivalul Slăniculu*i and *Toamna muzicală băcăuană* (The Slanic Festival and The Bacau Musical Autumn). These festivals revived ancestral customs performed over a large area starting with the village of Caiuți and ending with the village of Cătăfăneşti. They also promoted the old art of wood-carving in Bertunti or brought the beautiful dances and costumes in this region of the country to the stage' (Păun 1976: 39). Even more, some party essayists will try to present CR as being the crowning of *the* tradition itself: … 'in spite of all its novelty, beyond all its deep contemporary features, "Singing Romania", the large competition of work and creation going on in the country of Eminescu and Nicolae Ceauşescu, has very old roots' (Vişan 1980: 44). The festival is thus 'used as a symbol to signify the uninterrupted link between the revolutionary cultural tradition and the present political regime' (Giurchescu 1987: 165).

The Ideological Context

This obsessive reference to 'very old roots' has an important ideological reason: claiming national independence from Moscow, Ceauşescu and the party now had to offer another genealogy to the *new man*, one rooted in national past and specificities. In a paradoxical – and confusing – way, these have to be found in the peasant's traditional culture, the only one 'national' enough.[3] In order to use it for such a political goal, this has to be turned into 'popular culture' in the sense of the culture of the whole people.[4] This is important also because the incoming communist society was supposed to be a classless society, every socialist country having to prepare for this coming of age. Different solutions were found in this respect in different neighbouring countries at about the same time. In this context, Romania seems to have played, to a large extent, the card of staging the *whole people*, a kind of emerging society without social, geographical, or even professional differences: 'the CR had to produce a kind of cultural homogenization of the whole people, and to turn it into "one single living being", as we used to say', B., a former propaganda secretary of an industrial department, remembers. The agenda of the former 'national ethnology' offered a useful tool in this respect, cultural homogenization being an important stake for the nation-

[3] One of the first decisions of Ceauşescu's 'cultural revolution' was to forbid Western music on the radio and in restaurants, replacing it with Romanian pop/folk music and especially with genuine Romanian 'popular music'.

[4] Party activists and specialists have thus to define the complex relations between peasant culture, popular culture, mass culture and the like. In more practical terms, they have also to solve the problem of the 'integration' of peasant creation into the all-embracing popular culture of the whole people.

building project also. The rising national communism thus revisited the national agenda, looking for a common suitable patrimony to be shared by the whole people via a huge 'folklorization'[5] of cultural life. On the other hand, this same homogenization of Romanian culture was also a way of presenting, in advance, the future classless communist world itself, and thus to show to the world – and to prove to oneself – Romania's successes on the way to communism, in this respect outrunning its Eastern 'sisters'.[6] Socialist countries were in a 'socialist competition' too!

One should not infer from these observations the existence of an over-arching ideology, common to all the socialist countries and governing their cultural policies. There are, indeed, some common stakes and challenges, producing some similarities between a few countries. But most of them are solved differently by different countries, and, usually, at different times. In this respect, CR too was not the same when it started in 1976 and when it ended in 1989, a comprehensive approach of the phenomenon having to face this historical 'becoming'.

Finally, it would be misleading to look for a unique or supreme political reason or function of the CR. As in many other cases, the same political tool could be used in different ways and for different purposes. Some were added on the way; some others were by-products; and some might have been just unintended effects.

Summing up, one can claim that CR was not a totally unique strategy, was not totally the same during all of its existence, and it did not serve just one single aim all the time by all its components. Some of the activities developed in the frame of CR were over-reactions of zealous activists; whilst some of them were rather tricky inventions made by people having their work or interests promoted under the party's CR umbrella. More important for our concerns, this communist design of the 'whole people' the party was referring to was envisaged differently by anthropologists and other experts involved in the CR. To some extent one could even say that everybody was dreaming of his own 'people' that he wanted to stage, thus nuancing the oneness and unity of the ideological project: what the party loved to call 'the monolithic unity of the people' was not as monolithic as this.

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[5] 'State policies to preserve the cultural heritage often lead to the "folklorization" of culture, that is the development of selected elements of dress, music and oral literature, to be formalized for staged performances. Only truly national elements are included… state agencies promote a formalized folklore, selected, purified elements abstracted from any lived context and preserved as "heritage"' (Zubaida 1987: 155–6).

[6] The Party had already launched the concept of 'multilaterally developed socialist society' (*societate socialista multilateral dezvoltata*), meaning that Romania is closer – even if not yet close enough – to the final stage of classless communist society.

The Rules of the Game

In 1971 Ceauşescu returned from a successful visit in Korea that seemed to have impressed him very much. In July he presented before a party congress his new views on culture, known since then as the 'July theses'. A *sui generis* 'cultural revolution' was starting. But it would be misleading to consider that it was just an imported product or a kind of Korean dream of power: Ceauşescu only then felt strong enough to materialize his own ideology and long-lasting will of grandeur, the Korean episode being just a contextual impetus in the whole story, overstressing the spectacular dimension of this kind of popular manifestation. The emerging national festival 'Singing Romania', which would stage this vision was thus a complex ideological product, a strange mixture of nationalism and proletcultism, reminding one of both pre-communist and Stalinist times.

Five years after the 'July theses', the party held its Congress of Political Education and Socialist Culture in June 1976, providing for, amongst other things, the organization of the National Festival of Culture and Socialist Education. On 19 September 1976, *Scînteia*, the party's daily newspaper, published the *Programme of Measures for Enforcing The 9th Party Congress as well as The Congress of Culture and Social Education in the Field of Ideological, Political and Cultural Educational Work.* Point 8 stipulates for the organization of the 'Singing Romania National Festival for Education and Socialist Culture'. Some months later, on 28 November, the same daily published another document on the 'Singing Romania National Festival for Education and Socialist Culture, a brilliant manifestation of the love for work, of our folk creative vitues, expression of the democratism of cultural politics of the Romanian Communist Party'. 'I cannot judge how much of this may pass off as pure chance', Otilia Hedesan comments, 'nevertheless there is no doubt that the first version was a construction with a possessive case. Therefore "Singing *to* Romania" means singing for Romania whilst "Singing Romania" may be understood as Romania is singing, too. If the first version could be read as the Romanian translation of the old *Patrium Carmen*, the second version simply recalls Alecu Russo.[7] This last version embodies an entire ideology of the indestructible national unity around the party' (Hedeşan 2006).

The festival aims to be 'an ample educational, political–ideological and cultural– artistic manifestation of creation and interpretation. It addresses large masses of labourers and it aims to improve and diversify the spiritual life of the whole country, to enlarge the contribution of the Roma-

[7] A founding father of the Romanian nation, who's most celebrated writing, published 'anonymously' in 1850 in Paris, is called *Cîntarea României.*

nian folk creative genius to the national and universal cultural heritage.' The second paragraph adds three objectives. The first sounds just like the general aim quoted above: 'We aim to create a strong emulation in the field of artistic creation and interpretation, to stimulate the workpeople as well as gifted folk's creative spirit in order that all the arts and the whole culture become to a greater extent, a democratic movement, a way of expressing human personality. It should be an emulation of the masses and in the meantime their good.' The second one seems more precise: 'the creation of new works with ideas reach contents inspired from present realities, from our people's history, from the glorious fighting past of the party and of the working class'. The third one claims 'the increasing of the number of festivals and concerts, of the exhibitions and of other cultural manifestations opened to the public; the improvement of their contents and artistic quality in order to contribute to the education of the masses in the spirit of love for country and for the party...', and so on.

The festival addresses distinctively both professional and non-professional artists, of all ages and nationalities – 'artist' meaning in this case the 'creative mind'. This confusing and all-embracing mission confers an umbrella-like character to the festival, which sometimes evolved in quite surprising directions. Another confusing condition lies in the official statement that the 'right' to attend the festival is reserved to those having outstanding results 'in production, learning *or* artistic activity'. Otilia Hedeşan finds that 'this "or" is extraordinary. Talent comes after social and economic tasks are fulfilled – or it can be replaced by these achievements ... This "or" makes everything ambiguous.'

The contest is structured in two main fields, 'creation' and 'interpretation'. The first field comprises poetry, prose, dramaturgy, literary montages, folklore collections, journalism, music (revolutionist and patriotic, pop music and brass band), choreography, (ballet, new dance, thematic dance), plastic arts (drawing, sculpture, graphics, decorative arts, cinema and photography), popular art (costumes, tissues, sewing, wood carving, basketry, leather cutting, glass painting) contests. The field of interpretation is divided into theatre, revolutionist and patriotic music, brass band, classical music, folk music, pop music, ballet and thematic dance, popular dance, music hall, satire and comedy, artistic crews, dolls and marionettes theatre, literary montages and poetry reciting contests.

In time, there were more and more categories, many of them being split into sub-categories. The reason was simple: as many participants as possible had to be rewarded. The larger the participation, the more prizes were needed. In order to cover this need, the cultural activists started to split the different categories of cultural activities into more and more sub-

categories, producing thus more categories to be rewarded but also new and strange types of cultural actors and/or activities. 'They believed it was imperative to have more and more awards. In order to offer more awards, more bands were needed. However everybody was thinking the same way. What should we do then? We create thereafter more and more kinds of bands! This way they've got to a point in the field of folk dances where they had four or five kinds of bands. There were folk dance ensembles, folk dance teams, groups, and I cannot remember what else in order for them to be able to offer many more awards to more people' (C.).

Participation in the festival was linked to the economic competition between socialist brigades (which was in turn a key element in 'the carrying out of the party's programme of socio-economic development of the country'). Selection was a reward for outstanding labour performance, in the Stakhanovite tradition. It also implied moral excellence and educative commitment. In fact, it implied all the desirable qualities of the complex and complete 'new man'. The following statement by a party secretary of an industrial platform answering a *Scînteia* journalist in 1976 is typical:

> During the year 1976 the staff on the platform contributed a supplementary global industrial production amounting to over 17 millions lei and produced goods amounting to over 30 millions lei in order to accomplish our county's quinquennium plan 14 days in advance. We put into service a new furnishing factory, we produced 45 new kinds of goods and we designed, by means of self endowment, some pieces of equipment of great importance for factory production. This abundant balance sheet which we present in the *labour contest* of the whole country *entitles*, in my opinion, our staff to take part in the *educational and socialist culture contest*, the CR national festival, as well.

The festival had to cover the whole country in such a way that 'there were no towns, departments, villages, factories, institutions, agricultural units, schools or faculties without cultural artistic bands and creation circles. New talents belonging to the masses were discovered and an ample and diversified artistic and cultural activity was developed. This kind of cultural Stakhanovism also involved a strong expression of *socialist enthusiasm*. The official documents claimed explicitly that 'the festival has to express the *enthusiasm* of the working people engaged in the carrying out of the national quinquennial tasks', and 'the *fervour* of the whole people preparing itself for the great events of the next year'. The party wanted the whole people to enjoy, and everything had to be an occasion of shared joy. Enjoying the country's achievements was also a moral commitment every citizen had explicitly to make. And CR was the right and permanent frame to do it.

Ceauşescu himself summarizes this vision about the festival in the following way:

> One cannot conceive or understand a genuine CR without the tumult of the mine banks, of the top gases, of the rolling mills, of the heavy machineries, nor without the rustling of the work in the fields of electronic industry, of light and alimentary industry. Nor can one conceive a genuine CR without the buzz of the tractors and of the agricultural machines or without the enthusiastic work done by the labourers and by the peasants (*apud* Savonea 1979: 88).

'The whole country was supposed to be on holiday', C. concludes. One could even say that the more and more spectacular character of the festival was determined to some extent by this ideological and political desire for popular joy.

The regulations also stated a kind of 'cultural production' the festival had to promote: each artistic team had to perform at least one show each month; the theatre companies had to perform either three plays with one act or one play in more acts; the choral and orchestra groups were to play 6–8 new songs every year; the choreographic teams were required to increase their repertoire by 3–4 folk dances as well as 1–2 new thematical dances every year; the artistic brigades were to perform 2–3 newly released shows every year; the writing teams had to prepare 1–2 new shows every year, and so on.

Similar engagements were promoted in the realm of science and technology, each 'work collective' having to promote technical and scientific progress too, in their field of activity. As recalled by Iosif Tripşa, vice president of the Science and Technology National Assembly in an interview accorded in 1977 to a *Scînteia* journalist, due to the festival, 'the activity of technical–scientific creativity has (now) a permanent character'. In fact, almost everything was or could have been subject of this national contest, from technological issues to science fiction and from painting to a dog show. On 12 May 1980, for instance, the following activities were scheduled as part of the festival in the villages of Arad department, the village of Vîrfurile: 'Perspectives on the commune development' – round table; the village of Sinta Mare: 'Romanian people's fighting traditions'; the village of Bărzava: 'Political work for everybody, an important way of action for the workmen – mobilization for accomplishing the scheduled duties'; the village of Andrei Saguna: 'Science, the vital force for the progress of Socialist Romania'; Minis: 'Promotion of Arad department national heritage'; Buteni: concert of the 'Madrigal' chorus; Pancota: 'Industry, a decisive agent in the economic and social progress of our country'; Peregu Mare: 'the State as a political phenomenon in the contemporary epoch'; Siria: 'the 12th Romanian

Communist Party Congress regarding the development of the socialist democracy'; Felna: 'Ways and possibilities for decreasing production material costs, an important element for increasing the net production'; Ucutau: 'Spring in Romanian literature'; Varadia: 'Citizens ask, the scientific brigade answer'; Sad: 'Development directions for socialist agriculture during the quinquennium 1981–1985'; Bocsig: 'Recovering the human reject'; Bratoş: 'Order and discipline in the process of production, decisive agent in the livestock field' (*apud* Hedeşan 2006).

In order to achieve all these goals in all these fields of activity, the festival was organized jointly by a large number of state and party institutions, under the theoretical supervision of the Front for the Socialist Unity: Socialist Culture and Education Assembly, Ministry of Education and Instruction, Central Council of the Romanian Sindicates Union, Women's National Council, Central Board of Communist Youth Union, The Council of Romanian Communist Students Associations Union, The National Council of Pioneers Organization, The Ministry of Defence, Ministry of Interior, The Romanian Radio-Television, The Creation Unions. In fact, it was the Propaganda section of the Central Committee and the Committee of Culture and Socialist Education, with their departmental and local branches, which were ruling the game.

The festival started every second year with the mass stage, followed by the departmental one, the inter-departmental and the national one, and ended with a final gala in Bucharest, lasting several days, all according to a very strict agenda. The participants were advised all the way by different kinds of instructors and experts as well as by 'professional artists'. Their performance was judged at each stage by complex juries composed of 'specialists, party and state activists, members of mass organizations and representatives of the working people'.

These *rules of the game* had to be turned into reality first of all, mainly by the party's activists. Their first duty was literally to produce 'artists' in every each village and factory, school and institute. In the Galbinaş village, for instance, the local instructor considered that the small choir the villagers organized by themselves was far beneath their capabilities, so he helped (i.e. obliged) them to turn it into a 'representative' 100-person choir. The same in Ulmeni, another village where the members of the departmental jury helped the locals to set afoot a sixty-person choir, two popular dance groups and a thematic dance group (*apud* Dinulescu and Bazu 1977). When the cultural activists or other experts could not go personally into the communities in order to guide the people, they had to 'conceive *methodical letters* (*scrisori metodice*) to all the artistic teams concerning

their repertoire policy' (Căița 1977: 39). And the story goes on, the passage of the time being full of such examples.

In quantitative terms, this fervour produced thousands of new individual and collective 'artists' engaged in the 'Singing Romania' contest. Thus, a national statistic shows that on the first occasion of the festival, more than 6,400 theatre teams participated, plus around 5,000 choruses, 1,070 contemporary dance bands, over 8,300 artistic brigades, more than 3,900 creation circles and over 560,000 amateur artists. Ion Ilişiu, who later became the director of the Folklore Institute, presents the same figures in another way:

> More than 80,000 theatre, choral, folk dance and ballet groups, artistic brigades, orchestras totalling around 2 millions amateur artists took part at the first occasion of the festival. In the meantime plenty of literary circles as well as fine arts, folk art, cinema art circles and cinema clubs totalling more than 40,000 creators were established and impelled (Ilişiu 1977: 45).

Each department (*județ*) also praised its own achievements: 'at the departmental stage', states the secretary of the Satu-Mare departmental party committee, 'they staged over 4,000 shows', 'the number of bands grew from 387 to 620, plus 214 creation circles totalling over 3,000 members' (Căița 1977: 37, 38). His colleague from Caraş-Severin was also proud of 'the large participation, of the irresistible élan of those over 1,350 artistic band of the syndicates and of the cultural houses, totalling more than 20,000 amateur artists, over 800 school youth bands, totalling around 11,000 participants, 138 artistic creation circles, totalling around 2,000 members, hundreds of technical scientific creation circles' (Vela 1979: 54). These figures were proof that the whole people were enthusiastically involved in the festival. Sometimes, they were largely exaggerated: 'It was a moment when they amplified that much this fictitious reality so the number of artists in the town of Targoviste was greater than the whole adult population of the town!' C. remembers.

This dimension of competition should not be underestimated:

> CR is not only a festival, it is a contest and thereafter it should be thought about inside the whole system of competitions invented by socialism. Those who annihilate the free, the real competition, side with 'socialist competition', with the work competition. The contest is then generalized on the whole society level. In this way, they could achieve 'the art of production'. Art becomes quantified. It can be reported. They could count the winners of artistic production (Anghelescu 2006).

'The idea was that everybody had to be involved,[8] to be committed – and the instructors were responsible for this' (C.). Thus, as pointed out by R., 'it was not only a contest between "actors", it was a tough competition between instructors too'.

And, as in any competition, there were winners and losers…

2. Folk Experts and Living Patrimonies

Who was to lose and who was to win in this competition? What was it to win after all in such a contest in order for it to be worth getting involved? Of course, it was not at all a 'free choice', and the closer to professionalism, the harder it was for the participants: real artists and real scientists had to use their time and energy and, more importantly, to merge in this generalized populism. But the compulsive character of the CR cannot explain everything: for most of the participants there was something to win, after all!

The making of cultural VIPs

Taking part in the festival implied, first of all, being off the job, having 'free time'; for many villagers who never moved further then the nearest town, participating in the festival meant the fascination of real tourism. But more than just travelling, the competitions were always occasions of 'entertainment', of having fun and 'enjoying life', the 'parties' organized on such occasions being famous:

> in the beginning, they protested. Leaving home is such a dramatic thing for a peasant! Once they left home, they started having fun. Besides, they were travelling, which they hadn't done before. And they had spare time, which they had never had in their life. Moreover, during the breaks, they were having parties which challenged everything on the stage! Parties which could offer them the freedom they didn't have in their village. Finally, taking part to the CR was genuine delight for this folks! (Rădulescu 2006).

Dance assemblies had a different type of travel incentive: travelling abroad, as 'ambassadors of Romanian song and dance'. In a way, it was the highest award, open only to the CR national laureates. It is also the reason why 'popular dance' was the most attractive category in the CR contest, and why instructors and specialists were interested most of all in these juries. Next to this, popular singers also had their rewards, being presented on a stage or on

[8] As noted by Anca Giurchescu, in fact there exist no clubs or Houses of Culture where people can practise folk dancing or singing just for fun. Each of them must enter the *competition* and thus be subjected to control' (Giurchescu 1987: 166).

TV in the frame of CR meant more fame for them, and thus the possibility to ask for more money at the next wedding where they might play.

Another strategy to motivate participation was to give access to some 'scarce resources' such as gas cylinders for villagers, but also, for instance, some chemical substances for festival implied the need for chemists. Concerning handcrafts, 'CR allowed the department authorities to give wood for fire to the potters. For instance, the potters could negotiate with the forest districts in order to exploit the wood. There was a time when it was forbidden to work in copper. In Ialomița, for instance, Răzvan managed to get approval for working in copper and iron by means of CR. And so on…' (C.)

Last but not least, all of them were given public visibility and prestige, in an unprecedented way for most of them.

> I must say that the festival raised the village amateurs' feelings of surprise and joy… As proof, a lot of them who had reached a certain age, still feel sorrow about this. You can always hear: 'these were good times, man, we used to be excited, we used to rehearse, to leave for the town, in the centres…Did you know I saw Braşov for the first time in my life when taking part in CR?', somebody told me. The festival made them happy because they were seen as artists and therefore the village stars! (C.).

In the realm of folk culture, some of them were declared 'living legends'. They became real stars of their village, of the county, and some of them of the whole country who could see and admire them on TV. Recently, a documentary film presented a typical such case: 'Mama Ghița', an elderly peasant promoted by the CR festival as a peasant poet, won one award after another so that she finally was considered (and considered herself) as a 'living legend'. 'Eminescu has not as many diplomas as I have!' – she argues now, deploring the fact that since 1990 nobody has asked her to write poems and everyone has forgotten her. 'She just cannot understand what happened, why is she no longer considered to be what she used to be, a "total artist"! And there are many others like her…' (Ştiucă 2006).

The festival thus produced a social category of such VIPs, promoted as models and behaving as such. Thousands of persons, from simple peasants to intellectuals, from amateurs to specialists, were in this situation and find it hard now to give up their glory. On the other hand, it would be misleading to put all the participants in this category or to consider it homogeneous. In folk culture alone, there were noticeable differences between dancers and artisans, between popular poets and music interpreters and so on, each of these sub-categories having more or less their own destiny. 'The worst consequences have been concerning the popular costumes and dances; but popular art hardly suffered at all', Alina Ciobănel (workshop, 2006)

claims. Meant to produce homogenization, CR produced also unexpected differences.

The socialist market of experts

On the other hand, the organizers had also their own incentives. First of all, it was a competition between the leaders of the local institutions fighting to gain access to the top of the hierarchy and distinguish themselves. It started at the mass stage, where each unit, from kindergartens to large factories and from agricultural cooperatives to institutes of research had to have a 'participation score'. 'That is, if the X factory had a dance group, it could get 100 points, for instance; if it had a poem declaimer, it could get 2 points', R., a former chief of a municipal propaganda section, explains. It was all about participation. These points were totalled and afterward they established a classification. – It was the same with football players. For instance, you pretend that someone was in your team. In fact, he was from somewhere else. In the papers, he was a barber or something else. Nevertheless he was declaiming lines of poetry at the festival. Especially if it was for cultivated music, if he could creak on the violin the Ballad (Ciprian Porumbescu's ballad), everybody was claiming him, because he could bring points in the participation score. He was a scarce resource … How the in heaven's name could one find violinists in the bread factory!?' Ranking higher in this hierarchy was important for the career of each director and party secretary – and, to some extent, for the institution too. 'It was about the prestige of each factory. The bread factory, for instance, is awarded the first rank every year and this creates a good impression! On 23 August, at the national holidays or in the analyses they made at the centre, they were praised …They were awarded diplomas, medals, this kind of thing …' (R.). And all 'this kind of thing' was part of a kind of socialist CV, important for both individuals and institutions. For teachers it was part of their professional obligation and they would not be promoted if they were not involved in the festival and had written confirmation to this effect from the local party representative.

The competition went on to higher stages and was extreme at the final national contest. The political leaders of the department had to be present, also trying to obtain the favour of the jury. 'For the national phase, they used to bribe the jury. Each department was buying off for its participants through means of a good meal, of a bean feast. In this way the member of the jury could decently, properly go home … This was a mutual agreement. As far as I know they didn't offer money as a bribe. Some members of the jury happened to go home with a full luggage carrier … in case one should insist on a high rank in the classification …' Sometimes the 'lobby' was more sophisticated: a departmental institution bought, just by chance, a piece of art (a

painting, a sculpture, a text, a composition, etc.) by a member of the jury … 'It was a special kind of emulation, starting with the directors, all the chiefs were interested in the festival', R. considers.

On the other hand, in order to win, all these institutions competed for the best 'experts'. 'There were some gifted individuals, gifted at writing texts for the brigades, for instance. Others were gifted at leading a chorus, other for film directing. Usually these were the teachers of music, of sport…For instance, V., he was very good at organizing artistic brigades as well as at writing texts and directing. He was good indeed! D., for example, was a member of the department dance group. He was a sports teacher and he specialized in dance groups. And the dance group used to dance in a factory and to be paid for the show … Once he was told not to go anymore to anyone else because the factory could offer him more money. And if they could reward him by means of a bonus from the syndicate, they didn't hesitate to do it. The syndicate had special funds for such things. If not, he was paid in kind. I don't really know what exactly that factory did, but they gave him a chair, a cornflour bag … Nevertheless they gave him something!' (R.). Sometimes, an expert was the husband or a close relative of an employee, who was then given the mission by the director to persuade him to work for this institution. 'It was a real fight!', R. concludes. 'It was a permanent competition between trade unions and 'culture' too, all the time they were stealing the specialists from each other' (C).

The propaganda section of the party were obliged to provide all these 'experts' according to the needs and options of each institution, but it could not cover everything. In fact, the propaganda activists cared only for the 'small and weak' institutions, the larger and stronger ones looked for experts on their own. 'Usually the bosses made contacts with the experts in advance: "Hey, you, man, you are mine! I offered you a treat last year; I will keep you for this year, too." We [the propaganda activists] knew all of them very well all: this one is in the textile factory. We leave him there, we don't bring a new one' (R.).

In their turn, many of these 'experts' tried to sell their 'products': 'for instance, a choreographer who conceived a scenario either for a folk dance or for thematic dance, sold it. He was, for instance, from Cluj and he sold the scenario for 7,000 in Târgovişte, for 7,000 in Călăraşi … The same scenario. And they gained good money!' (C.). 'It was serious business. The juries were mainly made up of servile people. And they were staring at the bags they would pick up at the end of the show: meat, cheese … These servile people's loyalty was thereafter acquired by two means: social prestige and their pocket' (Rădulescu 2006, at the workshop). Consequently, one should not forget or underestimate the material advantages of participation.

In this context, 'expertise' began to be reproduced and exaggerated, especially in villages and small towns: 'Common village instructors used their brains in order to became great "creators". They were, how I should put it, obeying some fashions: flared trousers were fashionable in the towns. Whilst in the towns they were between 28 and 29 cm, in the village they were flared to 37cm' (C.).

From the local to the national level, one can say thus that there was a kind of 'market of experts': 'there was a competition between them, each having one's own quota, you know, just as if quoted on the stock exchange' (R.). Beyond the mere cultural activists, members of the higher or lower nomenclature (i.e. hired and paid as such), the festival produced in this way a social category of 'cultural experts', enjoying some material benefits also, but mainly social position and political recognition, and competing on the 'socialist market' for these incentives and prestige. It was a social category rather confident and proud of itself, having a certain kind of public legitimacy for the reasons we have tried to suggest above, growing larger and larger with the ongoing process of the festival, and thus gaining more and more momentum. 'It was a kind of chain reaction of experts-making' (C). Consequently, it was a social category that was rather hard to abolish or to reconvert after the fall of communism.

But what had trained folklorists and ethnographers to win in this national contest? The general view is that they had only to lose. To a large extent, it is true: they were losing entire topics of research censured by the party, and the liberty to choose how to approach the remaining topics; and they were losing time and self esteem in having to be committed in lesser or non-professional activities. But in the given context of the communist society, this common truth has to be revisited: compared with other social sciences, practically or formally disappearing from the public scene, ethnography and folklore had also to win something from its misalliance with the CR.

First of all, each of these specialists, mainly the members of the Institute of Ethnography and Folklore, was part of the larger community of 'experts' and was quoted separately on this socialist market. Their opinions mattered and could change the outcomes of the contest. If not really decision-makers, they were nevertheless important actors on the CR stage, competing also with the other 'experts' of the Romanian identity involved in the festival.

In a narrower respect, CR helped the institution (and the discipline) to survive and even to acquire small amounts of money for research needs: 'it was a big advantage, because we were protected against cut-offs. This CR was an umbrella which allowed scientific research to be carried on with the necessary compromises. Nevertheless scientific research could be carried on,

it did exist. It simply existed. There was no possibility of employing new people. Every assembly, there was somebody from the Board to shout: 'you didn't do your work, there is still work to be done, you should not do…' Nevertheless, the essence, the heart of the research, the heart of the important works, namely *The National Folklore Collection* and *The Romanian Ethnographic Atlas* were guaranteed. Every time they raised the problem 'we have no funds for research, we cut the funds for fieldwork, we do this and that', an answer came: 'it's not possible, you know, we need funds for CR' … 'Thereafter I believe that Professor Pop's trick of transferring us[9] really saved us in the long term. At least, and I repeat here, as funding of important research works' (Ciobănel 2006, workshop).

Further on, the 'affinity' with the party's propaganda apparatus could also be turned into a benefit when doing fieldwork: 'we turned to our advantage the fact that we were believed to be activists. We could thereafter use the relays of the creation houses, which was good. On the other hand we had relationships with the local propaganda departments and this gave us power over the village mayors. They could have been unfriendly hosts if we hadn't had the prestige of the ethnographers. Part of the prestige of the party was therefore transferred to the ethnographer' (D.). And more importantly, a part of its power – or, at least, a feeling of power.

But probably the most sensitive issue was the way each single individual managed to cope with their professional duties, whatever his/her attitudes and acts concerning the selection and promotion of folk culture in the frame of CR. Most of them think that they did their best, and thus a lot of things could be 'saved' and others 'preserved' due to their behaviour. Some also believe that they managed to use CR for the sake of the Romanian culture written large. All of them seem to share a kind of brotherhood in arms, having served as experts on the CR front and thus become members of a kind of corporate group – even if each of them may have suspected the others of misbehaving. 'Folk-experts', having shared more or less glorious exploits during the CR times, are thus a *sui generis* social category too, hard to convert and even hard to address by novices.

3. (Re)shaping Folk Culture

It was estimated that, at the beginning, about 40 per cent of the festival was devoted to the different categories of folk culture (Culea 1981). Its presence grew over time and became more and more visible, so that one could gain

[9] In 1970, the Institute of Ethnography and Folklore was transferred from the Romanian Academy to the Council of Culture and Socialist Education, which was perceived at this time as the beginning of the end.

the impression that it was mainly a huge country-wide folk festival lasting the whole year. CR was thus a huge process of (re)-shaping folk culture. But it would be misleading to consider that it was also convergent and totally planned, following some desirable final design of popular culture. Many things just happened during one phase or another of the contest, by one initiative or another; some things lasted, some others not; and, as already mentioned, some were just accidental by-products.

Staging and its consequences

The first and most important constraint of the festival was that everything had to be brought onto the stage, and thus hade to be adapted in order to be well staged.

From the very beginning, customs were chopped down to their festive aspects, independent of any specific contexts and thus totally changed their meanings: 'Every element in a custom staged can be considered as *kitsch*. No doubt about that. Even today you cannot explain this to anybody. This was indeed a great gateway to soiling folk culture, through means of CR. It was a giant gateway. Bigger, you know, than others' (A.). In this respect, Narcisa Stiucă recalls, for instance, the Goat mask custom: 'I found out from people who used to work in tutorial centres that the way they were staged at CR was imposed by their entrance on the stage. Because they should perform only for twenty minutes, all the Goat mask groups in a commune entered the stage all together. *Caiutii* who are known to have been performed by twos as in a mirror, are at present groups of twelve even twenty …' (Ştiucă 2006).

A better-known and more complex example is that of the *Căluş* ritual. It was transformed in (and reduced to) a spectacular dance with effect from 1935, when it was staged for the first time at the International Folk Dance Festival in London, later on promoted as a kind of national identity marker throughout communist times, and actually proposed as UNESCO patrimony by the Romanian Ministry of Culture. In socialist times, it was recognized as 'an expression of all that is full of life and liberty' (Ghinea 1957: 241). To this end, the *Căluş* dances have been completely divorced from their ritual milieu and are performed on stage as entertainment. The dances have acquired an identity of their own, representing a national aesthetic vitality. Almost every professional and amateur ensemble in Romania has its version of the *Căluş*, highlighting the dance's most dynamic and acrobatic movements' (Kligman 1999: 207–8).

Staging popular dance was, at first sight, easier. In fact, redesigning the choreography according to stage constraints was also a complex undertaking: because of the standard space of the stages that had to be full of

dancers and because the common constraints of getting on and off the stage, some types of dances were preferred and some other were reshaped in order to adapt to the context. Some local dances thus almost disappeared, some others were standardized into unrecognizable forms, and some dance formats were just invented. The music had to be adapted too. First of all it had to be 'pure Romanian': 'at the time they were even practising the "decontamination of folklore"! They adopted measures against playing something other than genuine Romanian because we didn't like, for instance, any Serbian influence. It was like this that time …' (Rădulescu 2006, workshop). Then, it was reframed for stage performances: 'it was performed by large ensembles in a standardized and repeatable way, following the direction of a conductor – everything totally different from the genuine peasant music it was supposed to stage' (*idem*). Some new 'fancy' ways of performing were also introduced, as for instance the fashion of duets. Last but not least, costumes had to be adapted too. They had to have ornaments, visible from afar, so that the colours were always very strong. They also were supposed to express local typologies, but in fact there was strong competition between the specialists (re)-designing them, as well as between the handicraft cooperatives that produced the thousands and thousands of genuine costumes. The final product was then ready for the CR stage. These dance and music arrangements were then presented on the only existing TV chain as genuine 'folk treasury' and thus turned to 'popular culture'. Some of the most performed ensembles were also staged abroad as presenting genuine Romanian traditions.

Reanimating traditions

The obsession with genuine old peasant folk culture in times when forced cooperativization and industrialization had deeply changed the conditions of life of the rural population produced a paradoxical effect: on one hand a lot of customs and practices were just dying out because of the new social and economic contexts, on the other hand many others were reanimated or invented by political request.

The case of the ethnically mixed village of Sirbi is typical in this respect. In the early 1980s, the Bulgarian community of the village started to perform the 'Bulgarian' ritual of *Sinziene* they discovered on the Bulgarian TV. The next year, some Romanian intellectuals from the village reacted, and a teacher started in his turn to train Romanian teenagers to practise the Romanian 'true' version of the ritual, using for this purpose a classical description from the early eighteenth century by Dimitrie Cantemir. The village won some awards at the CR contest by performing this ritual in an astonishing archaic and 'pure' way. In 1995, when a research team visited

the village, everybody presented the very much alive eighteenth-century version of the ritual as genuine and local tradition (see Răutu 1998). Every field researcher has similar stories to tell.

Existing folk patrimony, especially artefacts from museums, were frequently used for reinventing or reanimating local productions of pottery, costumes and the like. Sometimes it worked with astonishing results:

> Because a proper selection was made in some of the departments, kitsch was not accepted. People were forced somehow, besides the objects intended for sale, to introduce traditional techniques and forms. Thereafter these are not lost. I'm not suggesting that otherwise they would have been lost. Nevertheless I believe that the festival did contribute, to some extent, to safeguarding some elements. I can't deny that this safeguarding was done artificially, which I do not analyse here. At least, something was safeguarded. Regarding popular costume, whole areas were revived. I accept that there were new costumes being produced, which were copies of pieces in the museum. We closed our eyes to this. They came to the festival dressed in very old well-kept costumes. But one of the conditions was that the costume had been sewn between the two productions of the festival. Nevertheless we accepted the old ones in order to have them. We should have all the areas represented on the stage. I tell you the truth. They reconstituted some costumes. Mrs Secosan, especially, racked her brains to do this. I didn't find all the costumes well reconstructed, to tell you the truth, but at least they did exist. It was compulsory that they made costumes following traditional local patterns for the groups. One would ask in vain for the cooperation of these people in making at least a chromatic version or a sewing pattern …These were people one cannot talk to … In this way, at least two out of ten costumes ordered, were sewn according to the local traditional pattern. At least these two. I cannot judge these things as being right or wrong. I've told you from the very beginning I was going to present you with some facts. What happened next? They were forced to reintroduce traditional sewing patterns in the circuit. Of course I couldn't force some poor women who were dancing for ten hours to wear heavy hot hand-made cloth on the stage. They could faint and they were in danger of not having clean laundry for the next performance. It was thereafter obvious that we should have accepted a light industrial cloth … There were some areas revived, such as Baia Mare, for instance … The thing that X or Y was awarded a prize was shown as such. He was on TV and gained a kind of prestige which allowed him to go to different fairs and sell

> very good things. Therefore, CR was a kind of niche for some objects to survive. I believe these objects would have disappeared if it hadn't have been for the festival … (A.).

But probably the most important consequence was much less visible: with all this concern for folk culture and its genuine value, its former producers became, to a large extent, its present beneficiaries. Even if many of these customs and artefacts were no longer part of their 'real' life, they stood there as valuable symbols of the past and even as symbolic means of interpreting present life. The world-view embedded in these practices staged by the party with the help of experts has been thus reanimated to a large extent, competing in fact with the party's ideology itself.

The case of the *Căluş* is again revealing in this respect: 'In the interests of aesthetic and artistic appreciation, sacred ritual has been secularized into a spectacle of dance. The only ritual elements retained, the dancers' bells and *pinteni* (spurs), accentuate the hypnotic rhythm and driving intensity of the dances in any context. With the *Căluşari* on stage, the spectators are no longer participants; instead they are removed observers … In this manner, a new "religion", camouflaged as it is, is proselytized. Its message preaches life, vitality, progress. These ideals are held up in front of everyone as potentials to be grasped' (Kligman 1999: 208). They are also the ideals of the 'New Man'. But, on the other hand, preserving this staged and reduced *image* of the *Căluş* also preserves, in a way, the traditional meanings of this ritual as well as some of its practices:

> Driving through Mehedinți department during *Rusalii* (the period when the *Căluş* is traditionally performed), we stopped randomly to conduct impromptu interviews with the workers in the field. How did they feel about working for the cooperative farm on a day when, according to the tradition of *Căluş*, such work was prohibited? To our surprise, most people unobtrusively wore garlic belts, the ritual prescribed prophylactic measure against the harm to which they were vulnerable for violating the work interdiction (*idem*: 200).

Some aspects of the *Căluş* were thus turned to a disguised 'religion' for the New Man, while some others were readapted according to the old tradition way: 'During these years of intense socio-economic transformation, belief in one ideology or the other is at best ambivalent,' Kligman concludes (*idem*: 207).

By trying to reveal the 'true' form and 'real' meaning of such traditional practices (even if it was in some fake way and/or for the sake of CR), folk experts were thus reinforcing the symbolic function of traditions, helping to reproduce them beyond their social time and space.

Traditionalist over-reactions

> When doing fieldwork I found a cloth in the Olt Valley where I saw some tissue colours completely different from those which used to be spread in the area. I was curious to find out what exactly had happened. Around the 1930s somebody built a tissue mill machine and he had brought suitcases full of chemical colours. Apparentely the women rushed to buy these bright and beautiful colours. The effect was that the cloth was nice, sewn in a different chromatic pattern. The problem does not consist in chromatic itself. The problem consists in the way the colours are arranged inside the chromatic pattern. And the colours matched very well this pattern. They matched it in a different way. Nevertheless they matched it well. What is difficult is to make some people understand these things, to fight against some preconceptions! Whenever a brighter tissue was sewed, everybody backed down in front of it! Because they forgot two essential things: on the one hand, until the First World War, they used to use natural colours which are by nature pale. On the other hand, the well-known artefacts (folk art objects) which we all adore had grown old, unfortunately. Inherently, colours fade away when aging. Thereafter it was not the peasant who wished to have on a tissue, by all means, the pale green used by her great grandmother or the wonderful faint red! Nothing of the kind … She could get this and the rest withered in time. It was nevertheless difficult to make a bright object be accepted by the mentality of somebody who had only seen pale nuances. Today, things are the same… (A.).

Narcisa Ştiucă relates another case: 'I designed a glass display case under the name of CR at the back of the museum (Museum of Popular Costumes). And I wrote down on a label "stage costume" in few languages. The "purist" ethnographers reacted: "you must put away these costumes; take them to the stock pile!" I wondered why I should have taken them away as long as they were part of reality. Moreover, one can find them today in the Romanian village standing for an emblem of old times. And villagers look up to them' (Ştiucă 2006, workshop).

The common reason for these reactions – and many others alike – seems to be the fact that, taking the fight against CR mistreatment of the cultural legacies as a moral and personal duty, many of the ethnographers and folklorists (and also other amateur 'folk-experts') of the time were really trying hard to preserve and impose only 'authentic' (i.e. non-perverted by the Party–State interventions) expressions of traditional Romanian folk culture. Authenticity thus became a rather moral value of the much-praised intellectual 'resistance by culture', much beyond its supposed aesthetic roots. This

compulsive search for authenticity as such was thus 'frozen down the social life of traditions' (A.) and, in the long run, ensured even more the folklorists' and ethnographers' inability to approach social change and the inner dynamics of traditions. Instead of being just 'experts in traditions', they became more and more 'traditionalists' themselves.

Considering the ways CR modelled folk culture and moulded folk experts, one can come to the following conclusion: CR reproduced folk culture beyond its normal 'life-cycle' and produced a social category of folk experts larger and more committed then mere academic 'anthropologists'.

Final Remarks

CR was a *sui generis* brand of 'national socialist' modernization strategy, aiming to serve the coming of real communism and the building of the new man in Romania. In this respect, it was a carefully planned political endeavour – even if not everything was planned or went according to plan. And all the categories of 'experts' involved in this huge national contest served this plan, being thus, in fact, policy makers – 'folk-experts' included. In this respect, Katherine Verdery reminds us that 'this competition and its results are political' and that Romanian intellectuals – as any other intellectuals elsewhere – have competed for the Romanian cause for a long time: 'debates about something like "national essence" are contests through which intellectuals establish the social definition of what is culturally valuable – what is authentic, what scholarship is first rate, what art is superlative, and so forth – through judgements, evaluations, and discriminations that are produced politically' (Verdery 1995: 106). From the very beginning of Romanian nation building, 'such arguments about the Romanian Nation also served as a form of practical competition among those who considered themselves preeminent creators and defenders of the Nation' (*idem*: 105). To be sure, all these CR experts believed – and had good reasons to believe – that each of them knows the best what is good and right for the nation and that they are legitimate to claim it! Many of them still believe it.

On another hand, 'nation' and nation building were from their origins linked mainly to 'culture' and, even if serving the 'state' and state-building, they were not necessarily overlapping.

> Talk about the Romanian Nation served partly to consolidate state power by constructing a nation subject to state policies; it also represented the masses in a way that silenced them and opened them up to surveillance, control, and reform by state and intellectuals. At the same time, the discourse on the Nation created an ongoing potential for opposition to the activities of those in power, as it entrenched intellectuals within partly autonomous institutions from which they

could argue against the direction of state-making, justifying their arguments as a defence of the Nation's interest (Verdery 1990: 83).

A state-initiated project, CR was a 'cultural' project too and as such it also 'created an ongoing potential for opposition to the activities of those in power'.

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Chapter 5
The Politics of Tradition: Practices of Ethnological Museology under Communism[1]

Corina Iosif

Introduction

Just as is the case today, during the party–state regime, ethnological research took place under the auspices of various institutional structures. Due to the political–administrative super-structures patronizing them,[2] these institu-

[1] The present chapter is developed from the research work done within the project entitled *The Practice of Ethnography under the Communist Rule: The Romanian Case.* I conceived this project (the structure of the interviews and the theoretical argument) between 2003–4. Its first aim is to produce an *archive*, based on oral history interviews with senior researchers, regarding the actual *practice* of ethnography during communism in Romania. Although still influencing the present ethnological and anthropological discourses and performances, the *practice* of ethnography under communism (*unlike* the paradigm of the discipline) in Romania was scarcely researched up to this point. Consequently, the project's theoretical purpose is twofold: (a) to understand the practical, educational, theoretical, institutional and ideological conditions of ethnographical practice during communism, and (b) to clarify how ethnological inquiry today has integrated/rejected this practice (and, more widely, to clarify the status of this practice within the social sciences in Romania in general). Finally, the project aims to give precise content to the professional/academic debate about the role of this discipline in the recent past and in the present. In addition, it intends to quicken and concretize the reflexive query regarding practising ethnography, which began in Romania after the collapse of communism. In carrying out the thematic structure of the project I received useful observations from the anthropologist Ioana Popescu, the scientific director of the Romanian Peasant's Museum, and from the French anthropologist Claude Karnoouh. Beginning with 2004, Prof. Dr. Otilia Hedeşan, from the Western University (Timişoara), has been associated with this project; up to now most of the planned interviews were conducted by her. In 2006, Dr. Gabriel Troc (Babeş-Bolyai University, Cluj) joined our project, conducting interviews with foreign anthropologists (mostly Americans) who have worked in Romania. In 2004, the completion of the first set of interviews was funded by the New Europe College (Bucharest), and since 2005 we have received a grant from the Wenner-Gren foundation for conducting interviews with Romanian researchers.

[2] At the central level, this super-structure consisted of The Romanian Academy, The Ministry of Culture or The Ministry of Education and, during the last decade of the regime, The Council for Culture and Socialist Education, which was the highest agency of ideological

tional structures stood under the more or less overt influence of the ideology of national communism,[3] which constituted a significant element in the elaboration of their institutional strategies. Among these institutions, the research institutes of the Romanian Academy and the ethnographic museums accumulated the highest disciplinary and symbolic power. Ethnology was (and still is) one of the disciplines of fundamental research,[4] which were/are supported by the Romanian nation-state, especially through the research institutes of the Romanian Academy. Notwithstanding the increasingly 'post-national' features of current historical processes, both types of institution remain centres of major importance to the production of discourse on

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control, its ferocity being guaranteed by its direct subordination to Elena Ceauşescu. At the local level, this super-structure materialized in the so-called Centres for Guiding Folk Culture.

[3] In a broader sense, 'national communism' is meant to designate the proclamation of an autonomous self-comprehension, independent of the Soviet model, within the political-ideological project of the Romanian Communist Party (RCP). This variation became possible in 1964, when Gh. Gheorghiu-Dej attained the order from N. Hruschev that the Soviet troops leave the country. Such will to autonomy, to a certain 'specificity,' continued to affirm itself under the leadership of N. Ceauşescu, which began in 1965, and acquired increasingly amplified features under this regime due to his self-serving and strategic recuperation of important cultural values and references from the modern 'national' past of the country. But more important than this 'national-culturalist' nuance was his appeal to historiography (including the old nationalist and 'burgeois' one) as a purely ideological means for fabricating an increasingly problematic legitimacy. This appeal to 'national history' was based on two increasingly spurious landmarks of official Romanian history, considered to begin with the Dacian state: 'national unity' and 'unceasing continuance'. This tendency was at its highest with the 'July Theses' from 1971 and was consecrated in the 1974 programme of the RCP. For a detailed discussion of this matter, see Cioroianu 2003.

[4] I use the expression 'fundamental research' having in view the meta- or para-epistemological notion (and hermeneutic evaluation) of scientific research, according to which 'fundamental research' 'commonly designates an experimental and theoretical activity carried out in order to acquire new knowledge about the fundamental phenomena and observable facts without any practical application or utility' (Document of the Romanian National Committee for UNESCO, http://www.cnr-unesco.ro/ro/articol.php?id=24). This expression belongs (together with its counterpart category: 'applied research') to the commonly used vocabulary describing scientific research, as it was adopted also by the Romanian Academy in determining the research work supported by this institution. From the perspective of this categorial characterization, ethnology/folklore studies and ethnography are still fundamental research: 'there is a sequence of research, the academic one, within which fundamental research is carried out; this sequence deals with mechanisms, identifications, comparative structures, mental models, message structures and so on. We have behind us … a tradition of at least 150 years of careful research and we have, beginning with Haşdeu, thus with the end of the last century, even authorized academic research. Among the objectives that the old Romanian Academy designated for itself when it was established in the last century has been, together with the necessity of studying the language, the study of folklore' (fragment from an interview conducted by Şerban Postelnicu with Dr. Sabina Ispas, the director of the 'Constantin Brăiloiu' Etnography and Folklore Institute of the Romanian Academy, in Dialoguri, Contrafort, 5 April 2000, http://www.contrafort.md/2001/77-80/150.html).

national identity and, simultaneously, laboratories for the patrimonial construction of the nation state.

Under such circumstances, from the production of information by means of field research to the re-composition of this information in ethnological discourses materialized in texts, images, or – as it is the case with the institution on which I will focus in this paper – museum installations, the concrete practice of ethnology is constructed at the intersection of the disciplinary paradigm (with its historical preconditions), the existing political–ideological constraints and the individual experience of ethnologists. Even if, individually, ethnologists were most of the time under the surveillance of the political police (and at the beginning of the regime a part of the older generation of ethnologists was even repressed), the way in which the researcher, as the representative of a professional practice, negotiated his or her relationship with the ideological or institutional political restraints resulted in solutions and forms of disciplinary practice, which have today become significant for understanding a whole historical sequence of ethnology: the one dominated by the Communist regime.

On the Construction of the Ethnological Area of Maramureş

While the ethnography and folklore institutes of the Romanian Academy were the determining institutions for ethnology as an academic discipline, the second important aspect of the ethnographic discourse was constituted by the ethnographic museums, which were essentially of two types: the pavilion-based museum and the open-air museum. The history of these museums is complementary to the history of the folklore institutes. In general, this dual history was shaped by the interplay among the Communist power's programme (including the space of incertitude between the programme and the material practice of this power), the ideology of the national state, and the strategies of the persons involved in this interaction.

The open-air ethnographic museum in Sighetul Marmaţiei (Maramureş), established under the previous regime, can be considered a representative case because of the way in which its institutional dynamic was constructed at the crossroads of the model of its museological discourse, the relationship between institutions and political power (both central and local), and the interpersonal relations among the actors involved in these relationships. Building an ethnographic museum involves multiple negotiations at the intersection of these three dimensions, especially when the social–political system is so strongly determined by the ideological presence of the party-state. The particularities of the museum in Sighet, whose project and history I will try to describe, make this construction a representative sample of an entire epistemic practice. From this point of view, the history of the

museum's creation, and especially its result, the museum itself, is *representative* first of all because of the specific configuration taken by the relationship between the formal and the informal in the process of negotiation among the actors involved – negotiation that, in this case, constitutes the chief mechanism of institutional design. In other words, from the perspective of its history and its discourse – and, as such, of its significance – the particularities of this museum come about precisely through the way the institutional relations that led, in practice, to the existence of the museum intertwine with the personal ones. It will be shown that one finds here not only a fundamental condition for the practical success of this initiative, but also a very particular condition for understanding (the success of) this discursive and institutional construction. From this point of view, to describe analytically the concrete configuration of the museum means, indeed, to propose a reading of a significant example for a more general phenomenon: the institutional functioning of ethnological practices in Romania before 1989.

However, before analysing how the museological discourse is constructed, two particularities of the area 'represented' (constructed) by this discourse need to be mentioned. The first particularity resides in the importance accorded – independently from and prior to the creation of the museum – to the ethnographical area of Maramureş within the general ideology of the nation-state, when building the standard public image of 'the Romanian village'. Maramureş, or more precisely: its cultural image constructed through the mediation of ethnography (even before the coming to power of the Communist regime), had, beginning with the 1970s, played a crucial role in devising one of the Communist power's strategies, which, launching at the time the motives of 'ancestral origins' and 'national unity/identity',[5] had been interested in constructing in a determined fashion the relationship between the national and the regional. As Marianne Mesnil notes in an encompassing assessment of the situation,

> soon [after the establishment of national states in South Eastern Europe during the post-1918 period] a new preoccupation will appear: to identify the regional particularities. In Romania, the ethnographical area of Maramureş will be the one that becomes [in time] one of the privileged aims. Two discourses will intertwine here, illustrating the unity and the diversity of the 'Romanian peasant.' With the development of modernity, […] this regional specificity of

[5] The historical–political references to this are the July Theses (1971) and, especially, the new Programme of the RCP, adopted in the fall of 1974, within the 11th Party Congress.

> Maramureş will have the tendency to become the emblem of national identity[6] (Mesnil, in press).

This is the precise background against which the history of the open-air ethnographic museum in Sighet *becomes* significant – as I have already mentioned – in understanding the type of negotiation made possible by the actual relationships between the political power and the individual strategies involved in the construction and the functioning of institutional structures. Studying carefully this history, from the perspective of the import carried by its 'lesson' to ethnology, means, therefore, also to make some concrete steps towards the margins of negotiation, which, in practice, were made possible by applying the political–ideological monopoly of the party-state to both the formal and the interpersonal negotiation between the institutional actors.

The second particularity of this region lies precisely in the history of Maramureş. The origin of this history can be traced back to the fourteenth and fifteenth centuries, when, under the influence of the Hungarian kingdom, the patriarchal political organization characteristic of the Romanian stirpes [*neamuri*] was confronted with and progressively replaced by the political-administrative structures of *comitate* [shires]. This major administrative change was carried out mostly by the so-called 'donative' ennoblement[7] [*înobilarea donatară*] of the voivodal or knezial Romanian stirpes.[8] The ennobled peasant families retained until today the official insignia of their acquired nobility, the so-called 'dog skins' (famous in the region).

In 1918, a part of the *comitat* [shire] of Maramureş[9] passed to the Romanian nation state, which had been expanded to incorporate the provinces

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[6] Even if 'the interest in the peasant culture of this region appears relatively late, compared to other researched areas' (Mesnil, in press). As the author (following G. Oprescu, *Arta ţăranului român,* 1937,) aptly explicates in a note that accompanies this observation: 'The 'star regions' in the [paradigmatic, my note] construction of the discourse about 'national unity' were different, especially Argeş and Muscel, in the Southern region of the country.'

[7] '"The ennoblement of Romanian from Maramureş was nothing else than the assimilation of the prerogatives of voivods and knezs into the corresponding levels of the feudal hierarchy and the investment of their [own] lands with the privileges of nobility." This process of nobilitation was part of the strategy and tactics implemented by the Hungarian kings in order to win the loyalty of the major Romanian families from Maramureş' (cf. Alexandru Filipaşcu, *Le Maramureş,* p. 12 *apud* Dăncuş 1986: 19).

[8] It was in the same period that 'the majority of the localities and families from Maramureş' were registered in the documents of the Hungarian kingdom's chancellery (cf. Dăncuş 1986: 17).

[9] Since 1733, the region had been part of the Hungarian kingdom, and then of the Austro-Hungarian Empire. However, beginning with the sixteenth century, the ethnic composition of this area became more and more heterogeneous by settling Saxon and Hungarian colonists in Maramureş. The region was affected by another short episode of integration in the Hungarian state between 1940 and 1944.

within the Carpathian arch. After the Second World War, the rise of the Communist regime challenged, in its turn, the functioning of the communitarian systems of this area, especially by expropriating the old families from Maramureş and by centralizing the economy. As regards ideology – in spite of the Stalinist–Leninist precepts, or intersecting them – once it comes to power, the regime continued to develop in practice the principles of nationalism that have, since the eighteenth and nineteenth centuries, built the shared national identity of the Romanians from Maramureş. Whatever were the social and political contexts forging this identity throughout the history of Maramureş, one of the most important components of this history was the social structure whose frame and logic was given by the order of stirpes [*neamuri*].[10] After the first stage of establishing Communist power (characterized by the so-called post-national(ist) rhetoric of class struggle), the ideology of the post-war Romanian state drifted from the recuperation of the nationalist discourse that drove, during the nineteenth century, the creation of the nation state (in which regionalism played an important role), to a discourse that built the Romanian state as a unity and, since the 1970s, recalibrated national identity by levelling the images of regionalism. During this process, the discourse on regional features became a discourse on national features. However, in Maramureş, the social and historical reference point of identity has continued to be built upon the notion and representation of the solidarity of the stirps [*neam*], as a particular form of regional identity.

The Museum

The beginnings

The ethnographic museum in Sighetul Marmației contains a pavilion section (called the Ethnographic Museum from Sighet) and an open-air section called the Maramureş Village Museum. A brief history recounting the main events of this institution runs as follows: its existence begins in 1926, when a local ethnographic museum is established in Sighetul Marmației. This museum will cease to exist in 1940, after the region becomes part of Hungary, and its collections are mostly lost. In 1956, ten years after the the Commu-

[10] In its broader sense, the *'neam'* [the stirp] designates the bilateral kinship: *kindred.* In the sense in which it is used here, *neam* means a social system of the type of *à lignage* societies. H. H. Stahl describes this type of social organization as having 'a clearly 'genealogical' character, not only in the sense that the inhabitants [of the village] stated that they constitute one main descent line, resulted from one eponymous ancestor, supposed to be the founder of the village, and, which is more, in the sense that these inhabitants would regulate their patrimonial rights as regards the totality of the lands pertaining to the village according to a genealogical tree sometimes memorized, sometimes written' (Stahl 2005 [1969]: 49.)

nist regime came to power, the Ethnographic Museum from Sighetul Marmației was established, and in 1981, as an extension of the above-mentioned pavilion-based museum, the open-air museum, called the Maramureş Village Museum, was created. The latter is the institution upon which we will focus; however, the various meanings of the museological discourse practised by this institution cannot properly be described if considered outside of the history of the Ethnographic Museum from Sighet, i.e. outside of its general institutional dynamic.

The establishment of the Ethnographic Museum in Sighet took place during the period when the Communist regime had almost completed the collectivization.[11] However, because of the inter-war precedent for its existence (the ethnographic museum inaugurated in 1926[12]), the 1954 event was interpreted as a re-establishment.[13] Between 1954 and 1957, when the first exhibition in the current building of the museum was organized in a very small place, the institution existed only in the documents attesting its existence. During the same years, Francisc Nistor, the first director of the museum, managed to recover partially the collections of the first ethnographic museum.[14] Up to 1967, the new museum functioned with only one exhibition, displaying collections of art, ethnography, and natural sciences.[15] The stock of objects was, however, very heterogeneous: 1,236 objects 'of all

[11] Moreover, one of the most feared Communist political prisons was located in Sighet.

[12] Cf. *supra.* In 1860, with the establishment of the Society for the Culture of Romanian People from Maramureş, there first appeared the issue of creating a museum in Sighetul Marmației. The collection of documents and antiquities of Ioan Mihaly de Apşa (1841–1918, lawyer, historian, archaeologist, member of the Romanian Academy, the offspring of one of the most important stirps *[neamuri]* from Maramureş; he published in 1900 *Diplome maramureşene din secolul XIV–XV* [Maramureş Insignia from the fourteenth to fifteenth century]), as well as the establishment in 1878 of a city museum with two sections; a historical and a natural history section preceded the more focused institutional undertakings regarding the creation of an ethnographic museum. After 1921, the year when Maramureş joined the Romanian state, an ethnographic museum of this region was created in 1926 through the efforts of Gheorghe Voinicu. This museum had four sections: extracting crude oil/ shepherding, domestic customs, home industry, and church art. The goal of the museum was 'to preserve the spiritual treasure of the Romanian people from Maramureş, to inform and educate the public [in popular arts]' (cf. Dăncuş 1986: 31).

[13] Cf. Dăncuş 1986: 31.

[14] As a consequence of the Vienna Dictate, which led to Maramureş being integrated into Hungary, many collections of the museum were lost. Some of them were destroyed, while others became parts of private collections. The collection of rugs and carpets was partially saved. (M.D.)

[15] Interview with Mihai Dăncuş, the director of the Ethnographic Museum from Sighet. The interview was conducted in May 2006.

kinds, rocks and minerals, stuffed birds, pressed plants, a few archaeological pieces and a very important stock of ethnographic objects'.[16]

In 1968, the museum was closed for restructuring. This was the year in which the current director of the museum, Mihai Dăncuş,[17] took office at this institution.[18] In 1971, the pavilion-based museum was officially opened, and their exhibition-discourse – 'no labels' – would remain unchanged for more than thirty-five years.

At a retrospective glance, one can already formulate some general observations about the context within which the museum from Sighet was opened. Its establishment continued the strategy of patrimonial construction, which, at the beginning of the twentieth century, marked every historical region of Romania: in 1927 the Ethnographic Museum of Cluj, conceived by Romulus Vuia, was established; in 1936 Dimitrie Gusti founded the Village Museum in Bucharest, and in 1940 Ion Chelcea established the Ethnographic Museum of Iaşi. The model of museological discourse constructed by these leading institutions relied as a source of legitimacy mostly upon the way that peasant culture had been integrated into the project of the Romanian nation state at the end of the nineteenth century. At the same time, by the specific way it relates to the rural world, the museological logic somewhat recuperated the interventionist perspective of the Bucharest School of Sociology.

The project of the pavilion-based museum in Sighet does not constitute an exception. Its discourse was constructed through the placement/display of the exhibited objects, which created a static image of the 'tradition', without opening up many opportunities for new interpretations. The construction of the museum was supported by three strategies, which became its main pillars: recognizing, preserving and handing down authen-

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[16] *Ibid*.

[17] M. Dăncuş is the descendant of an old stirps from Maramureş. When he was five years old he witnessed the capture of his grandfather (who eventually died in prison) by the forces of Securitate. In the last year of high school he was expelled because he took into school Romanian literature that was banned under the censorship of the regime, but also because he was the son of a Greek Catholic priest. After he had reapplied for high school and graduated, he attended an accountancy school, because he was refused admission to the Faculty of Architecture for the above-mentioned reasons. He worked as a teacher in one of the village schools from Maramureş, and later on he applied for the Faculty of Philology in Cluj, attending courses in the first years of his career as an employee of the Ethnographic Museum. Most of the information supporting this paper comes from a conversation with Mr. Dăncuş, who was so kind to meet me for a research interview in 2006.

[18] At that time, the personnel of the museum consisted of four employees: 'Feri bácsi [uncle Feri] (the first director, F. Nistor), Jóska bácsi, András bácsi and Rózsa néni [aunt Rózsa]'. Among all these persons, only the director, Francisc Nistor ('Feri bácsi'), possessed the qualifications of a museum curator, the others having been hired as auxiliary personnel. (M.D.).

ticity. The solidification of this discourse in its original form is inscribed into the logic of its construction. Nothing changes, everything is given, and everything needs to be saved, preserved, and handed down.

The Professor and the centre

The 1970s, the years that followed the opening of the pavilion-based ethnographic museum in Sighet were marked in the Romanian ethnological school by the campaigns of field research initiated by Professor Mihai Pop.[19] Most of these field research trips took place in Maramureş. At the time, the research teams consisted mostly of students, but among the participants one could also find professional researchers. During the same period, Professor Pop brought to Maramureş anthropologists from the West, who chose Romania as their ethnological field. In the 1970s, anthropologists such as Jean Cuisenier, Claude Karnoouh, and Gail Kligman (to mention only those who continued working on Romania after the fall of the regime in 1989) started their field research in Maramureş. They also had an intimate knowledge of how the Sighet museum was built. Mihai Dăncuş, who at the time was newly employed by the museum,[20] also participated in these campaigns of field research. As a consequence of his participation in the research, in 1969, M. Dăncuş organized an ethnological conference in Sighet, which constituted the beginning of a series of annual meetings in which not only the researchers of the Ethnography and Folklore Institute from Bucharest participated, but also other academics, and writers, etc. (interview with Mihai Dancuş).[21]

This series of research trips initiated by Professor Pop marked the professionalization of a whole generation of ethnologists at the beginning of the 1970s.[22] One of the reasons why Mihai Dăncuş participated in the research

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[19] Between 1965–75 Mihai Pop was the director of the Ethnography and Folklore Institute in Bucharest. A central figure of Romanian ethnology under the communism regime (see the chronology accompanying the present volume), he presided together with A. J. Greimas and J. Cuisenier over the first International Congress of European Ethnology that tool place in Paris in 1971.

[20] Otherwise, at the request of Professor Mihai Pop, Dăncuş took care of the organizational issues relating to field research. (During one of these campaigns, because of too few rooms to accommodate the researchers, he sheltered almost forty students and researchers in the houses of his relatives from the area!)

[21] The majority of the participants came from Bucharest, influenced by Professor Pop. For this reason, the Cluj-based folklorists were mostly not present. These meetings took place on a regular basis during the campaigns of field research initiated by Professor Pop in Maramureş (1969–74), and later on more and more rarely.

[22] 'It was a real joy conducting these research trips and an unimaginable satisfaction. We all came with our small discoveries, which we considered spectacular, and in the evening we would share them with the Professor and with the others. He would listen to us with an

teams formed by Professor Pop was precisely his overt interest in the development of the Museum and especially in the establishment of an open-air museum.

The presence in the region of Professor Pop's school of ethnology lent to Maramureş a new dimension as a privileged ethnological field. Some of the foreign anthropologists that he brought into this region have stayed to continue their field research. (It was precisely during the period when Romanian ethnology, dominated for a long time only by folklore studies, started to speak about structuralism.) The concept of an open-air ethnographic museum as an extension of the pavilion-based museum in Sighetul Marmației was developed against the backdrop of divergent interests, which had turned the region not only into an ethnological field that became a reference point for Romanian ethnology, but also into an exotic field for Western anthropology.

The concept of the open-air museum

From its very conception to its materialization, the open-air museum in Sighet owed its existence to Mihai Dăncuş. He dedicated the most important part of his professional career to this project.

Dăncuş mainly used two types of experience in carrying out this project. Firstly, the field research in which he participated together with Professor Pop's teams (although later on he would also undertake field research trips by himself). Secondly, the courses of specialization[23] he attended until

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incredible generosity, with patience, and he would give us courage. He gave us the best he knew. We are his creation, everybody in his or her way … We decided the structure of the interviews, and then we discussed them with him, applied them, and developed them. He taught us how to formulate the question, the question within the question … We devised the questionnaires, but he would give us the ideas. He just suggested, he always just suggested…' (interview with Mihai Dăncuş, May 2006).

[23] The courses of specialization (called in the jargon of the era 'recycling') took place once at every five years, and their agenda was conceived as a combination of information from all fields that could be relevant for the work of a museum curator. 'It was the Communist system, but it was very well designed. It made you to study the new research methodologies. When we started to study structuralism and Lévi-Strauss, our professor was Mihai Pop. Later on the courses were taught by Paul Petrescu and Károly Kós, and they covered ethnography and field research. Sometimes Sanda Golopenția and Constantin Eretescu would come as well. There were professors from every discipline, bizantology, museology…' (interview with Mihai Dăncuş). The programme was supported by the Ministry of Culture and, later on, by the Council of Culture. These courses, besides their importance in the professional formation of students, brought to the participants the benefit of contact with various figures of an intellectual elite liked by the political regime and also with figures of the Communist elites. Thus, they became an important opportunity for the practising and wearing-in of the relationships among the participants of these courses (who were at the same time the representatives of the

the regime changed. The main idea behind the project of an open-air museum was to create the 'model' of a traditional village characteristic of Romanian villages from the fourteenth and fifteenth centuries:

> a theoretical model that I tried to validate in practice. I wanted to rebuild, and to <furnish> this model using artefacts discovered by the field research. As opposed to other Romanian museums, I intended to return to these artefacts their original functionality. This is what constitutes the main value of our museum. The Village Museum [of Bucharest] started with a notion of sociological museum elaborated by Gusti; the Museum in Cluj is based on Vuia's idea of live museums, in which people are still living (before they had even sheep and cows there); Vuia was the founder of the ethnographic school … Later on there appeared a series of open-air ethnographic museums displaying peasant architecture, where the most beautiful houses from a region were exhibited in a haphazard museum, without any logic other than the aesthetic one (M. D.).

The project of this *model* of a representative village had four main sources: an insignia from the fourteenth century 'representing a burning village', found in the collection published by Ioan Mihaly de Apşa,[24] the cadastral maps[25] included in the travel diaries of foreigners, especially Hungarians, the *Ethnographic Atlas of Romania*,[26] and field research, 'which was the most important element'. Mihai Dăncuş began the latter during the campaigns of field research initiated by Professor Pop (1970–4), and, in the first stage, he covered the work of sub-zoning the region. This work was carried out by correlating the maps of the medieval knez-ships with the ethnographic and dialectal sub-zones of Maramureş, all with their specific peasant architecture. It was a collaborative work undertaken with two other participants of the field research campaigns organized by Mihai Pop: Radu Popa, who was interested in questions of medieval archaeology, and Paul Petrescu, who carried out the part of sub-zoning that was based on the criterion of peasant architecture.

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various institutional structures – the museums from the country), and the representatives of the above-mentioned categories.

[24] Cf. *supra,* n. 14.

[25] Mihai Dăncuş mentioned during the interview the importance of the cadastral maps made by the Austrians in the nineteenth century to the understanding of the developmental dynamic of the various localities: 'today's villages are packed. Before you could see only a house on a hill, another one quite far away… There are cadastral maps made by the Austrians; nobody studies them; you can find them all over Transylvania; even the trees are marked; a house here, a windmill there…' (M.D.).

[26] This is a major work initiated and completed by the Folklore Institutes of the Romanian Academy in more than one stage and over many years.

Later on, Mihai Dăncuş continued the research in a solitary fashion as well. As he himself puts it:

> Do you know what I did? I didn't have a car, and I would come by foot to Sighet. I would take the bus to Mara, to Crăceşti on Valea Mare, and would come by foot, when I managed to get there … The next day, the same! Or I would sleep there. During the summer, as I told you before, the houses were open; you cannot imagine this thing! Thirty years ago the houses of the peasants stayed open all the time, they would go to work the field, and they would leave them open. I would go into the pantry, and I would take a bucket of cold, heavenly buttermilk – I was so thirsty and hot… sometimes I would leave a note: 'Thank you!' Q – Did you take any pictures? Or did you make any drawings? A – Thousands! Tens of thousands, I don't even know how many! (M.D.).

Although their intention was rather to search for and identify the pieces to be included in the collections of the museum, the research trips carried out in the region where Dăncuş was born took the shape of discoveries with a true acknowledgement-value. Of course, this is an anticipative acknowledgement of the presumptive patrimonial significance of the various pieces (their 'authenticity'-value), but also, in this way, an acknowledgement in the sense of a reconfirmation of identity (both for the objects and for the researcher). At the same time, going back to the field causes the researcher to take into consideration the time parameter and, implicitly, the change. (In this case, either the change is negatively assessed and becomes loss, the research acquiring the aspect of recuperation, or the change is integrated into the process of constructing the 'tradition' and, implicitly, the patrimony, leaving behind the paradigm of folklore studies.)

However, during these research trips, Dăncuş became aware of the fact that the old, wooden architecture had been replaced by a new one, influenced by the social changes that took place after the instauration of the Communist regime. The abolition of private ownership of land resulted in a redirection of investments made by local people towards building houses. Nevertheless, a major chasm appeared between the style of building characteristic to these later houses and the local architectural tradition. Once started, this process continued after the second political regime change, the one that took place during the 1990s.

> The small amounts of money that they had gained, since they no longer had the ownership of the village, were invested in building houses. They would readily leave behind their splendid wooden houses, these remarkable pieces of architecture created over the centuries and displaying the most advanced technical and artistic skill.

> They arrived at a point when they would burn their old houses, using the fire for producing the bricks required to erect the prototype of house characteristic to the period of socialism. Today, this prototype is on the verge of disappearing as well, for the influences coming from the West led to the demolishment of these houses, and from the building materials acquired in this way, people would make new houses, using different models, taken from here and there (M.D.).

Thus, the establishment of the open-air museum, therefore the preservation of traditional type(s) of houses, became the objective of an emergency ethnology.[27]

The houses and their inscriptions

Finding the proper location for the project[28] allowed for the installation of the first houses. In 1974, says Dăncuş, there were already two houses, and in 1981, the year when the museum-village was opened for the public, the village had ten houses and a church.[29]

According to M. Dăncuş, two of the most important and manifest criteria for selecting the architectural pieces (the houses and their annexes, churches, etc.) were the following: (1) their age, proved beyond any doubt; (2) the existence of inscriptions on them. Today, the museum has a house from the sixteenth century, four houses from the seventeenth century, and more than twenty houses from the eighteenth century – authenticated beyond any doubt. From this point of view, the Sighet museum is truly unique in Romania. The houses have a known history, which is incorporated into the museological discourse – this being the element that legitimates their presence in the space of the museum:

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[27] Before the establishment of the museum, there were experiments to preserve the houses *in situ,* but this project failed.

[28] The area considered to be faithful to the image of medieval village from Maramureş was based not only upon Mihai Dăncuş's field research, but also on the image reconstructed from the cadastral maps. Purchasing the land gave rise to serious administrative matters, which delayed the building of the museum. It was land that belonged before 1918 to an Austrian who died leaving no descendants. As after 1918 the Romanian state did not create the necessary documents demonstrating the ownership of the land, sixty years later the patch of land could not be bought for the museum until the whole sinuous administrative procedure of state ownership was completed, in order to resell it later to the local authorities – the whole process took almost two years (M.D.). This situation demonstrates the importance carried by that specific piece of land, which matched the pre-established model of a 'Maramureş village' (even from the point of view of the landscape), in the accomplishment of the museum's project.

[29] The architect Elisabeta Lucaci conceived the architectural project of the museum.

> For instance, I found a house […] which was made out of an old wooden church dating from about the end of the fourteenth century, the beginning of the fifteenth century. The church had been demolished during the sixteenth century. They took a couple of beams from the church and built a house! In an instant, the image of what that house looked like and of how people lived during those times becomes clear … A very interesting house from the region of river Iza's ford (the Arba house – named after the family who owned it), which was inherited by two brothers during the last centuries, suffered quite a dramatic fate after a disagreement regarding the sharing of the property. They cut it in half with a saw. One room remained within the original footprint of the house, while the other was moved to a different location. Later, to both halves they added new rooms. I have found the first half of the house, the one with the doorframe still intact, and later on I learnt about this story from the people around. This is also how I identified the other half of the house, in a different location. I bought this second one, too; I eliminated the added parts and I brought the complete house to its original shape (M.D.).

Regarding the second factor of authentication, the inscriptions one can find on every house of the museum, the author of the museum says:

> It is very significant that all the houses bear such inscriptions, as opposed to the Village Museum [of Bucharest, m.n.], which has only two or three of such houses. One of them is from Maramureş and it was built in the 18th century, all the rest are from either the nineteenth century or the beginning of the twentieth century. The inscriptions are very important. They are monuments of language, even if they consist of two or seven words only. I still have beams recuperated from [various] houses … I bought doorframes and central beams that would have special decorative elements explicating their meaning, as well as inscriptions in Latin, Romanian or Cyrillic, or dates written in Arabic numerals…Some of them are extremely interesting: 'This house was built in the year of God 1736, during a famine and when a measure of corn would cost…' this many florins. Or 'this house was build when the judge of Ieraşu de Sus was pan Saplancai' – Săpînțan was one of the old noble Romanian families, but the name was written in Hungarian; or when 'the priests [parohuşii] of the village were Ioan and Mihai Mihalyi.' Therefore, all of these inscriptions record important events … Other very interesting inscriptions are: 'This thing was (here the word 'made' lacks) in 1785, the beginning of Dechei [December], 29 days'. Or the Uirca

> house from Călineşti, owned by the family of my great-grandfather, from the peasant side of my family: 'From the beginning of the world, year 7301' or: 'Year of God 1798, June in 21 days, the day of saint martyr Iulieanu.' Or another one: 'With the help of God they worked on this house of the ecclesia – of the church – at the advice of Pavel Săpânțan, the judge of Ieraşu de Sus, by the efforts of Chendrij Mihai, when the priests of the village have been Ioan and Mihai, and the leading workmen Chindriş Dan with Pleş Ionaş in the year of God.' Or: 'At the advice of the Father and with the help of the Son and the working of the Saint Spirit, craftsman – this is in Romanian spelled in Cyrillic – Paşcu from Sălaşe asks you to mention him in your sacred prayers. This house was made by Pop Gheoghe in the year of God 1775, the second Thursday after the sacred Palm Sunday'; and another one: 'Edificavit 1704 filius Wolfansi' (M.D.).

In the 1980s, when the official ideology regarding national identity was already building the discourse on an increasingly homogeneous national identity, the open-air museum from Sighet launched from the very beginning (1981) an equivocal and ambivalent museological discourse, presenting an image of tradition constructed simultaneously on two levels of meaning. The first level reflected 'the regional specificity of Maramureş as a mirror of national identity' (Mesnil, in press). The second would construct the meanings of region/locality upon the basis of the historicity of its discourse, a historicity whose keys of thorough intelligibility have been accessible only to those familiar with the history of the region. Therefore, the historicity involved here was a local historicity, overlapping the ethnographic construction of the same locality.

In integrating concrete historical temporality into the strategy of museum construction, by selecting the pieces to be included into the collection according to precise dating parameters (cf. *supra*) this temporality is not displayed in order to reflect a factual succession in time. What takes place is the redirection of the conventional ethnographic discourse on 'tradition', which postulates an 'atemporal' 'tradition', towards a discourse that transforms the 'ethnographic testimony': this discourse evolves not so much into a source of (local) history than into a 'historicist' legitimization (i.e. using the authority of history) of *constructing* this testimony. This is, indeed, the way in which the museological discourse participates also in the construction of local identities. Let us tarry for a while on the way in which these dimensions of the museological construction intertwine.

The logic of the identity discourse proposed by the open-air museum in Sighet is based on the construction of the historical dimension of tradition

as a factor of its legitimacy. But this is a truncated, incomplete temporality of a local historicity (one which can be asserted only in contrast with the one that supposes the official historical fiction of 'national' identity). This history begins in the sixteenth century and ends in the twentieth century, when the inhabitants of Maramureş started replacing their wooden houses with new ones, whose style attests to the abandonment of previous models. Presently, and this is quite revealing, among the thirty-eight units of the museum, there are a Hungarian house, a Ukrainian one, and a Jewish one, which complete the model (the ideal image of a Maramureş village) with signs of ethnic plurality, represented in a symbolic proportionality. The ethnic proportionality evoked by this setting is not considered from a statistical perspective, but on the criterion of exemplarity, from the perspective of recuperating a compressed regional history, as such idealized.[30]

Furthermore, the setting of the space is simultaneously decoded/ explicated as a reconstruction – within the discourse on the symbolic dimension of the place – and as a construction, within the discourse on the elaboration of the project. This procedure, at first ambiguous, reflects the two layers of identity discourse upon which the project of the museum is built. As a laboratory of patrimonial construction *par excellence*,[31] the museum still remains within the paradigm of producing a discourse that is solely indirectly faithful to the ideology of national identity, because of its contamination with the phenomenon whereby the image of Maramureş arrived to symbolize the generic picture of the Romanian village (Mesnil, in press). However, the content and meaning of its practice deviate from the model of this discourse, because it is not 'national identity' that is the object of the identity building, but 'regional identity.' This identity claims as its origin not the Dacian state, existing at the beginning of the first millennium, but a medieval history. From a symbolic point of view, this medieval history is constructed as recent history: the houses have owners, and the genealogy of their ancestry can be traced back to their origins. These origins are rooted in the medieval age, in a period during which the existence of the Romanian families in Maramureş has been attested. In fact, collecting such representative houses is precisely the *programmatic project* of the museum. We have here the most important linking element in the concept of this museum, the principle that structures the hierarchy of the various meaning-levels of museological construction, articulating and gathering them together by

[30] The image of ethnicities related to the history of Maramureş is thus presented in a manner that summarizes a historical development stretching along a couple of centuries. In fact, the Jewish population of Maramureş diminished considerably beginning with the middle of the twentieth century.

[31] Botea 2005: 85.

constructing a *locus*. The ethnographic discourse, the historical reconstruction and the identity factor support each other by the fact that eventually the museum in Sighet is a 'monument' of the most important stirpes from Maramureş. The houses and their placement within the space of the museum already constitute something similar to a map of these stirpes. In this sense, the author (and the curator) of the museum will say:

> There is a very important institution to Maramureş, which even today cannot be neglected, although it is perhaps considered simply a relic, a paradox of history: the institution of the stirps [*neam*] … The names of the houses are the names of the great stirpes of Maramureş, which lived in certain areas of this region and, because they didn't find any equal partners in their place of origin, searched for them outside their own communities and, in this way, all become families since the medieval period … This village [i.e. the open-air museum] is the village of the villages and stirpes of Maramureş. All the former owners of these houses were relatives. It was not an accidental choice, but a programmematic one to select exactly these houses for the museums. Like it or not, this is the reality, this is Maramureş … This is the story of the museum.[32]

The historical construction proves, therefore, to be derivative of the identity-function of the museum, and the construction of locality appears eventually as an act of commemoration (and celebration) of an 'ideology' of the stirps [*neam*].

> By this [the institution of stirps (*neam*)] Maramureş resisted the shocks it received along history, and especially after the Second World War. And it still resists today, in this period of confrontation. Before, the confrontation took place between close political regions, ideas and dogmas […] – today, Maramureş is in a confrontation with Europe, and it is meant to resist precisely because of the mentioned institution (interview with Mihai Dăncuş).[33]

Therefore, the museological discourse should be read in layers. While to the untrained visitor the museum spontaneously offers an image that resembles other discourses on ideal rural settings, for those having a trained eye, but

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[32] This quotation is from the speech held by the director Mihai Dăncuş on the occasion of the twenty-fifth anniversary of the opening of the Maramureş Village Museum – The Open-air Museum from Sighet, May 2006, Sighetul Marmației.

[33] The constant presence of Professor Mihai Pop in Maramureş and, implicitly, his participation in each stage of building the Museum could be interpreted in the same paradigm of the 'ideology' of the institution of stirps *[neam]*: 'Q – How did you meet Professor Pop? M.D. – Naturally… he was part of our stirps. We are in a way a family, and the old intellectuals of Maramureş know each other … Today, chance has us meet again and recognize each other as coming from the same stirps' (M.D.).

being from outside Maramureş, the reading of this context focuses on the history of the region and its particularities. For those who, in one way or another, have access to the general equation of the museum, especially those whose history is evoked in the construction of the museological discourse, or the ethnologists familiarized with the project of the museum, the deciphering takes place at the meeting-point between the construction of group identity[34] (the stirps, *neam)* and a local identity: Maramureş.

Usually, the main criterion for selecting pieces to be exhibited in ethnographic museums is the 'beauty' of the objects. However, there is not much talk about how these 'aesthetic' patterns are constructed during the process of patrimonial construction. I would only mention that the ideology of 'aesthetic excellence' relies on the image of 'the celebrating village'.[35] Thus, the extraordinary (and, as such, representative) event of the rural community becomes an ideological source for building a model of excellence (specificity) integrated into the discourse on national identity. However, in our case the so-called 'aesthetic' criterion in the elaboration of the museological discourse is left aside (cf. *supra*, the statements of M. Dăncuş about the criteria used in selecting the houses), the determinant factor that actually structures the space of the museum being the symbolic construction of the large kinship networks from Maramureş.

The museum and the political power

> Q – As a matter of fact, when did you attain the official document attesting the establishment of the Museum?
> M.D.: Never.
> Q – Not even after 1989?
> M.D.: No. Not even today have we an official approval of (establishing) the open-air museum. All kinds of institutions, for instance the revenue authority, ask us for the official document attesting our existence. But we don't have it (fragment from the interview with M.D.).

The history of acquiring legal status for the museum in Sighet shows the nature of the relationships between the political authority and the institutions patronized by it. The comprehension of these relationships presupposes an understanding of the interactions among the individuals affiliated with these

[34] Otherwise, some of these units and objects arrived by donation from the own family patrimony of the director: 'I have put together a small collection of paintings. They were all given to me, for myself. I inherited them from my uncle, Traian. I gave them all to the museum; I kept none of them' (M.D.).

[35] M. Magiru, director of the Ethnographic Museum from Constanța.

two spheres: on the one hand, the political structures, on the other, the institutional structures – both central and local.

There were many attempts to legalize the existence of the museum. Through the actors participating in the procedures, all of these undertakings were redirected from the path of legality to that of informal recognition. Every step of this process activated interpersonal relationships, which discretely but systematically replaced – along already beaten tracks – the institutional procedures.

The first stage of these procedures took place in the year 1974–5:

> I elaborated the thematic structure of the open-air museum; in 1974 I finished up the project. I went to Bucharest having with me a sketch of the thematic structure and a report necessary for the authorization of the project – because, of course, you needed an authorization. They created a new institution for that and the necessary authorizations for cultural institutions were emitted by the propaganda section of the Central Committee of the [Communist] Party, on the basis of a report by the Ministry of Culture. At the time, Tamara Dobrin had been the vice-minister – a very intelligent, very educated woman – they said many bad things about her, because she was a passionate woman … It was wintertime. I remember that besides the food restrictions, we had also restrictions on energy… It was freezing cold in the Casa Scînteii [House of the Spark].[36] I entered the office of the chief officer, I told him who I am and I asked for an audience with the minister. The general director, Ioan Antonescu, came with me; he was an exceptional person, with several special merits in building the museum network in Romania; Mr. Opriş, a state secretary of the previous government, was there, too. We entered the office of the vice-minister. I can see her before my eyes even today, as she stood behind a huge desk, having a fur coat on her shoulders, and all shaking because of the cold … 'Speak Dăncuş!' – due to smoking, she had the voice of a baritone – she used to smoke a lot. 'Here they are, I brought along the documentation, as I have already told you…' – I had met her before, she came to one of our courses of specialization and she lectured on a matter I forgot – she was at the Faculty of Philosophy.[37] She looked at me and then she looked at the other two, and I saw them apologizing and leaving the office. She stood up from the ministerial chair, she took my arm and we left her office. We passed through the office of the chief officer, and got into the

[36] Today, it is called: Casa Presei Libere (The House of Free Press). It is an impressive building, designed upon the model of the Lomonosov University from Moscow.

[37] See note 25.

> hallway. These hallways are enormous. And then she told me: 'Dăncuş, take your files – I quote quite literally – don't leave any scrap of paper around and just do whatever you please. If you leave any scrap of paper here, you won't be able to do anything.' 'Thank you so much, good bye!', I said, and I left. Why? Because there were no previous authorizations for open-air museums in Romania, and it didn't take long for them [the power] to demand even the removal of the Village Museum from Bucharest. In between, we changed a couple of ministers; they tried for years to attain the removal of the museum, and later on they gave an order, all in capital letters, coming from the office number two, where the lady-comrade [i.e. Elena Ceauşescu, m.n.] was settled, that excepting the Village Museum, which will have to move soon, all open-air museums in Romania are abolished.[38]

Thus, the official customs of establishing the museum were not passed, but avoided. This chain of circumstances is significant in at least two respects: firstly, it makes clear the way the organizations of power functioned inside the party structures. Secondly, it shows the distance between the general decisions made at the highest party levels and the way they were communicated and eventually carried out. In essence, as opposed to a simplistic model of the functioning of 'totalitarian power', the complication and, thereby, the relevant fact to be emphasized here stem from the parallelism and, consequently, the short circuits, the non-coincidence between such a party-state regime, i.e. the hierarchy of political functions, and the institutional order *per se*. The short circuits take place, when they occur, on two axes: (a) the one defined by the relationship between the central and the local power; (b) the one defined by the juxtapositions between the political realm and the institutional one. In this way, interstices appear, opening up a space for the practical negotiation of decisions. On the descending ladder of the power hierarchy, and on the ascending one of the latter's institutional clientele, the political or administrative decisions could take different shapes, in accordance with the personality, interests and personal beliefs of the decision-makers. The ethnographic museums did not escape this equation.

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[38] An anecdote related to this decision refers to a visit of Elena Ceauşescu at Rîmnicu Vîlcea. The story is told by Cornel Bucur: '… the prime secretary, Coman, took her to see the Bujoreni museum and the lady, realizing that the museum was in a grassy orchard, asked what plan of economic production they have. The man was embarrassed: 'I don't think that they have a plan, but, you know, the museums don't have any…', and she returned to Bucharest and gave a decree, which I read, too: all the open-air museums should be abolished and their lands will be recuperated for the sake of agricultural production' (fragment from an interview with C. Bucur, conducted by Otilia Hedeşan).

The second type of difficulty in the attempt to create the museum in Sighet was the acquisition of the financial means to allow the beginning of the construction works. The source of these means was the local administration of the county [*județ*],[39] constituted by the County Council of the Party, which was patronized by the First Secretary of the county. The finances were easily obtained, after a deal offered by the First Secretary himself, who was personally interested in the matter.

> The First Secretary from Baia Mare was a wise man, calm and with a good judgment. He said …: 'Sir, we approve the establishment of a museum in Sighet, if you help us create one in Baia Mare as well.' In this way, tacitly, without any official approval from other institutions, we began to build the museum from Sighet. They approved some funds for us … After that, since the order was given and the blueprint was finished, everything else has been carried out (interview with Mihai Dăncuş).

In general, the relationship between the [state] power and the ethnographic museums seems to have been, in its turn, ambiguous. While, officially speaking, the top hierarchy of political power seemed to consider the development and establishment of open-air ethnographic museums a senseless endeavour, these museums became, around the 1980s, some sort of 'protocol institutions'[40] of the Communist Party's elites in power.

At the same time, the museum directors, situated at the interface between the sphere of political power and their own professional environment, as well as at the intersection of two simultaneous roles (ethnologists and museum administrators), evaluated justly the place occupied by ethnographic museums in the general equation of the ideology of national state and of the public discourse built upon this ideology.

> The political, therefore, the power, needed this type of institution for various reasons. The museum from Sighet … was potentially of a high value to them, and they learnt this through their intelligence service. Consequentially, the museum became a place where they would invite their guests from the power hierarchy. Notwithstanding where these guests were coming from (West or East), the party activists would take them to Sighet, because they didn't have any other place where they could take them. They couldn't show them any-

[39] Territorial-administrative unit in Romania.

[40] 'Our museum had become some sort of a protocol institution of the C.C. [Central Committee], where figures such as heads of state would be invited, and they [the party leaders] boasted about the fact that we so brilliantly demonstrated the *raison d'être* of a society based on Marx's doctrine' (interview with Cornel Bucur, the director of the Museum of Popular Technology from Dumbrava Sibiului; the interview was conducted by Otilia Hedeşan).

> thing else, because they wouldn't take them to churches. So, if the guests wanted to see religious icons, the party would bring them to Sighet, and I would lecture for them on these icons (interview with Mihai Dăncuş).

In other words, the museum had the advantage of offering a constructed image of the rural space, re-directing the attention of the viewer from the reality of the Romanian village to its idealized image, which, in its turn, would construct the discourse on national identity in this way. Therefore, without being fully subordinated to the official political discourse, this image has become convenient.

The link characterizing the relationship between political power and the museum is not that of an institutional patronage (from the core of the Communist political power to the patronized institutions), but that of an (inter)personal patronage, since most of the time the personal relationships of the museum administrators with the representatives of power would guarantee the preservation of the institution itself. About one of the directors of the Village Museum from Bucharest it was said: 'He withstood more than thirty years at the head of the Village Museum's administration … He had his own particular tactics, and he stuck with them until he left: he never asked for anything, he didn't bother anybody.'[41]

In his turn, Mihai Dăncuş tells the story of his own tactics: 'If I had an idea that I wanted to carry out within the context of the museum, I would repeat it so many times to the decision-makers that they would think it is their idea, and then they would give me the order to execute what I, in fact, have suggested.'

In this light, the political decisions (and their effects) do not appear as simple univocal results of the mechanisms of 'total power', but as outcomes of the negotiation between individuals, some of them situated on various steps of the political power's hierarchy, some of them representing the interests of the patronized institutions. The museum, which had functioned in fact in semi-legal conditions,[42] was simultaneously less and more than an

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[41] Interview with Geta Stoica conducted by Otilia Hedeşan.

[42] After meeting Tamara Dobrin, helped by Georgeta Stoica, Mihai Dăncuş attained indirect recognition for the open-air museum – an official document that mentions that it is approved 'to expand the Ethnographic Museum, the permanent exhibition of the Ethnographic Museum from Sighetul Marmației'. It is not mentioned, however, what this extension will be and where it will take place. This document had no real utility, for the open-air museum was never integrated into the official list of ethnographic museums from Romania. (Geta Stoica is one of the prominent figures of Romanian ethnography. During her years of professional traning, she worked at the Village Museum from Bucharest. She collaborated with Professor Mihai Pop and the French ethnologist Jean Cuisenier. She worked for the Ministry of Culture, in the department dealing with the administration of museums, and later on at the Commission of

institution. It functioned as a place, as well as a *locus*, where the – intellectual or political – elites would often come, and this situation legitimated its existence and invested the museum with symbolic power. In these circumstances, even opposition to the will of the representatives of political power could be imagined, but only within the limits and conditions of an established/known discursive game, which was, because of this, allowed:

> They asked for a corner with Ceauşescu in every museum, notwithstanding their profile. At the entrance, they required a huge portrait of the leader, with laurel leaves, flowers, slogans, quotations, and the rest … The propaganda section of the party demanded it. They demanded of us the same thing; of course, we didn't do anything. They came to check on us: 'Where is the Comrade?' I told them: 'If you wish to place his portrait next to *opinci* [peasant laced moccasins], wooden ploughs and devil masques, you're free to do it. I can't do such a thing.' And that was it! (M.D.).

The museum's relationship with the power was periodically negotiated with the regime's officials, and achieving a happy end depended on versatility in negotiation. Failures were due rather to the incapacity to manage interpersonal relationships than to ideological disagreement. If the interpersonal relationship was good, the representative of the power would, most of the time, not intervene in the decision-making:

> If you happened to find a wise man or woman, he or she would let you be – if this was not the case, then you were hindered in your goals. I will give you an example: in 1984–5, the secretary for propaganda was a woman. If you wanted to organize an exhibition, you had to produce first the thematic structure. This structure needed to be approved. The first time I went to her, she looked at it…flicked through it, and said: 'Comrade Dăncuş, I understand nothing of this. You know this very well. Take care of what you do. When I went for the second time, she treated me the same, so I never went again (M.D.).

Legitimating the museum: television

Full recognition of the open-air museum's existence followed the same logic that had been applied in the establishing of the museum: it took an informal path as a practical solution. The solution came precisely from the media centre of Communist power: the television.

Historical Monuments. After the change of the regime in 1989, she was for a while the director of the Village Museum from Bucharest.)

> And then our escape came: the television, which was the media organ of the party. They realized the value of the museum. One day I met a team, I talked to them and, indirectly, they told me that they planned to create a centre of practical demonstrations, of craftwork, following the model of folk art cooperatives. The show lasted two hours, and I spoke, I [reported] from this centre of practical demonstrations …This was the scam the Romanian Television applied in order to conceal the idea that the museum is a village museum (M.D.).

Almost every ethnographic museum in the country was at some point required to produce a script for a TV show. The image of the museum was integrated in this way into the public debate on national identity and, implicitly, on mass culture. From this point of view, the initial image of the museum became subject to manipulation. This manipulation was already incorporated into the first TV show and took the shape of a replacement: the public rendering of the dissimulated image of the museum (as 'a centre for practical demonstrations of folk art') replaced the public recognition of its existence.

The images produced in this way were integrated into a stock of recorded media images, therefore, into a different discourse, which was archived and utilized according to a different logic than the initial museological discourse. The new process of patrimonial construction that started in this way can now be read according to a new table of meanings, proper to the construction mechanisms of mass culture. The ideological dimension of this process can and must be directly linked to the political strategies to which it belongs.

Conclusion

Along all its stages, the history of the museum takes the shape of a series of ambiguities. The first ambiguity resides in the programme itself of the institution, the stakes of which are simultaneously located in a national–regional dual picture. Sometimes it is the national that dominates, sometimes the regional, according to the viewer's focus. From the local viewpoint of the insider, the museological discourse points to a regional identity, while from an exterior viewpoint the same discourse constitutes a metonymy evoking a national reference system of identity. 'The country is like Maramureş, but the latter is not exactly as the country' – this is what this inverted metonymic figure[43] seems to say, while the 'whole' is (or, at least, seems to be) ex-

[43] The suggestion of formalizing the cross relationships among the 'national' and the local stakes of the museum discourse of the open-air museum in Sighet in terms of an 'insidious'

pressed by the 'part' only to assert, discretely but insistently, the *particularity* of the part. The ethnographic construction of Maramureş into a cultural image that overlaps the ideological construction of the country actualizes, in fact, its own meaning by recuperating and subverting the latter's import to the benefit of building the regional character.

All these registers of signification (which in order for the metonymy to function, cannot ignore one another) answer a certain horizon of expectations: firstly, the horizon of the political power (of its ideological programme and its project of society); secondly, the expectations of a larger and more ambiguous circle of actors, whose minimal common feature is the sentiment of belonging to the national (comm)unity. At this point of interpretation, however, in order for the metonymy to be fully intelligible, the relationship to the identity of the nation ('the national' as the result of a *nation-building* process) needs to be dissociated from the proper political dimension of the nation-state and, which is more, of a Communist party-state (*state-making*).[44] In the described case, this dissociation (which does not mean full disconnection) imposes itself on the analysis precisely because the building of the regional character (and of locality) happens directly upon the model of an image of the 'national', which is supposed to be already constituted and at hand. The building of the regional character is directly grafted onto this model. For what links the model to its actualization seems to be first the stake constituted by the sentiment of belonging to a 'national' unity, whose reference point has its origins in the time before the instauration of the Communist regime. In brief, the unity in view is not the unity of the nation (in the political-ideological jargon of the party: 'the unity of the people' – searching for a synthesis between *Volk* and 'the nation of workers'), but the unity of the 'stirps' [*neam*].[45]

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metonymy came from Vintilă Mihăilescu and was occasioned by his reading of an intermediary version of this text. I am also grateful to Adrian T. Sîrbu for his careful reading of this text at every level of its composition and editing, helping me to further clarify my intentions in key moments of the demonstration.

[44] For the distinction between *nation-building* and *state-making,* see I. Iliev, V. Mihăilescu and S. Naumović's reading of Verdery: 'Talk about the Romanian Nation served partly to consolidate state power by constructing a nation subject to state policies; it also represented the masses in a way that silenced them and opened them to surveillance, control, and reform by state and the intellectuals. At the same time, the discourse on the Nation created an ongoing potential for opposition to the activities of those in power, as it entrenched intellectuals within partly autonomous institutions from which they could argue against the direction of state-making, justifying their arguments as a defence of the Nation's interest', (see the Introduction to this volume).

[45] The term *'neam'* (translated here by 'stirps') has more than one meaning in Romanian, designating both 'kinship' and 'nationhood': '<neam> [literally: *kindred:* the totality of the relatives] = 1. *popor, națiune* [people, nation]; 2. The totality of persons united in a family by

Another dimension of the ambiguity incarnated by the institutional construction of the museum from Sighet is to be traced in the complexity of strategies (managerial, cultural, professional, political) presiding not only over the establishment, but also over the management of the museum in its relationship to the political power. The rules of this game need to be recuperated first of all by analysing the way the interpersonal relationships are constructed, which seems to be decisive (i.e. operational) for the materialization of the institution's project within the general relationship between the political authority (the warrant of an ideological order given by the regime) and the actors of administrative strategies (at both central and local level). This practice of connecting in the described way – in *raccourci* – the administrative decision to ideological planning (or, the negotiation-character of the decision) could go as far as the *justification* of the museum's public and administrative existence *outside* its legal sanctioning. Under the circumstances of the previous regime, this coincided *de facto* with the lack of *clear political approval* (although not completely independently from the latter, because the institution functioned illegally but not clandestinely! – which, after all, clears a little the symptomatic ambiguities of the ideological project regarding 'the national').

Finally, a third layer of ambiguity characterizes the equivocal relation the political power had with the patronized cultural institutions (in our case, with the institutions whose vocation is the process of building a patrimony rooted in peasant cultures). In the analytic formalization of this ambiguity one should not ignore an apparently paradoxical situation that may be described as follows: while, within the social project of the Communist Party, peasant societies find less and less a place, in the elaboration of the ideological discourse, the power stayed constantly interested in the *confirmation* of an already built image of 'tradition' – as a fundamental, irreplaceable element in constructing the discourse on the nation-state as a 'unitary national state'. To simplify, one can say that traditional culture was considered as an 'inheritance' (and a 'monument') rather than as an 'instrument' in the reproduction of social life (or a 'document' of the latter). This paradoxical situation translates itself into symptomatic attitudes and conducts of power regarding the ethnologic practices and their formalization within institutional structures. While the cultural images that may serve as emblems of the 'tradition' are constantly and acutely in the political power's field of inter-

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blood or alliance; the person who belongs to the same family with another' *(Dicționarul explicativ al limbii române* [*The Explanatory Dictionary of the Romanian Language*], second edition, Bucharest, Univers enciclopedic, 1996.) However, tracing an equivalence between stirps *(neam)* and nation *(națiune)* dates back to the time before the Communist regime came to power.

ests, the latter is not interested in the intra-disciplinary dynamic of ethnological studies or in the epistemic procedures that build these emblematic images. Moreover, the political power seems to be indifferent to the institutional shape of ethnologic practices (of course, as far as the potential knowledge production of these practices does not compete with the vision on society heralded by the party).[46] This situation triggered the fact that, *from the perspective of the political project concerning peasant societies* (and under its pressure), the main function of the folklore research institutes of the Romanian Academy, as well as of the ethnographic museums, was the collection, classification and presentation of the specific categories of peasant culture (the creation of typologies). In principle, the work of researchers seemed to stop at this point – at least from the perspective of public visibility and political orders. According to the same political programme, the 'capitalization' of this labour has been rather a task of mass culture's ideologists, belonging to the project of building mass culture. At the same time, one should not forget that, by the absence of sociology from the landscape of social sciences (excepting a short intermezzo that coincided *grosso modo* with the period of relative liberalization after 1965, sociology was clearly rejected by the regime), ethnology remained the only discipline that stayed in contact with social reality in general and with that of the rural world in particular.[47] While the political power seemed to be truly interested in field research, in fact: in its *results* – and only as such in the way they were carried out (giving the impression that it is interested in ethnologists and ethnography at the core of their professional practice), the truly interpretative intellectual production resulting from this practice could take place only in a marginalized way. (This is why even if the interest of power in this discipline is limited to those pieces of knowledge that can become ideological means, the political surveillance of ethnologists could have been, almost all the time, a very strict one.) The general complexity of ethnology's relationships to power and the ambiguities characterizing these relationships (on

[46] This lack of interest went so far that one could talk about the abolition of all open-air museums, except the Village Museum from Bucharest, and the relocation of the latter to a highly marginalized area in the public space of the capital. This lack of interest regarding the institutions of ethnologic research can be perceived also in the prestige status of folklore departments at various universities, hosted usually by faculties of philology. These departments have been perceived only as a second option of the valuable young philologists, in case they didn't manage to be employed by more prestigious departments. (This observation was made by Otilia Hedeşan on basis of a series of interviews conducted with professors from the folklore departments in the country.)

[47] For this reason, ethnology remained the only discipline in contact with the social reality as form and method of knowledge. This was undoubtedly one of the important reasons why the ethnologic field research constantly attracted the attention of the Communist political police and its ideological control.

both sides) were the conditions that opened to ethnology, as practice of a scientific discipline, a wide landscape of negotiation in finalizing some of its projects – those that would not overtly contradict the ideological programme of the regime. In this context, an ethnographic museum without legal status no longer appears as an anomic entity. Its existence depended on the possibilities opened up within the wide space of negotiation that I have mentioned.

Under circumstances wherein there is no wide gap between the need for transforming the products of ethnologic practices into political instruments (the agenda of power), on the one hand, and the cultural discourse of the ethnologic institutions (the cultural activity), on the other, even if the two do not completely match, a crisis does not emerge.[48] A relevant clue in this sense (and simultaneously an intuitive criterion of the limits of this gap) is the fact that on the path that leads from the conception of the project to the establishment of the museum and its development into the shape in which the museum acquired and consolidated its legitimacy, even without legal authorization (except an informal one), there were no fundamental political penalties or administrative obstacles to face. These circumstances suggest that the social space within which the existence of the museum became possible – at the intersection between *nation-building* and *state-making*, to use again the terms of the distinction launched by Verdery (cf. *supra*, n. 45)[49] – can be described as a space of structural connivances, in spite of the various actors' agendas. This is why the contextual negotiation of particular projects was made possible. (For a more accurate rendering of the conditions of possibility for this space of negotiation, it would be necessary to take into consideration some additional parameters, which I can only evoke here: the regional importance of the object of negotiation in terms of visibility and/or exemplarity, the proximity/distance between this object and the core of power, its symbolic eccentricity, etc.) The dialectics of a possibly divergent relationship between *state-making* and *nation building*, which from a logical point of view is quite promising as an interpretative framework, cannot be

[48] The counter-case is to be found in the example of the Commission for Historical Monuments of the Ministry of Culture, which was simply abolished in 1977, after the specialists forming its team, on the basis of ethical and professional reasons, did not approve the demolition of churches from Bucharest as having a patrimonial value (interview with Geta Stoica conducted by Otilia Hedeşan, 2006).

[49] I. Iliev, V. Mihăilescu and S. Naumović: 'as noticed by Verdery in the Romanian case, nation defending institutions may work successively as well as simultaneously for as well as against state institutions and strategies. Thus, anthropologists, as 'national scientists', were not necessary the apologues of *the* Nation, but could have been in different power positions or, as in the case of socialism, have a double talk by playing the 'nation' against the 'state'.' (see the Introduction to this volume).

used in the present case without certain precaution. First of all because it projects from the very outset the sphere of *nation-building* as a meaning horizon of 'opposition'[50] (be it even virtual) to the politics of the party-state. For this to be the case, for this space of negotiation to credibly represent a possibility of interpreting the museum discourse of the open-air museum in Sighet as dissidence or heresy in relationship to a certain cultural policy programme, one would need to demonstrate that the party, *at the time* when the establishment of the museum had been approved (1974) and, *a fortiori*, after this moment, in the 1980s (cf. *supra*), 'did not know what it was doing'. In other words, that through the political decisions following the path of *nation-building* (be they even discrete, masked as administrative tasks), which have been made by the high functionaries of the state, the party created – 'unwillingly' – this space of virtual contestation of its programme in the realm of *state-making*. (In this respect, an extremely pertinent question is whether or not such an institutionalized cultural project would have any chance to succeed before this moment!) From the perspective of the history of changes in the political line of the party, the moment of authorizing the establishment of the Maramureş Village Museum by Tamara Dobrin coincides with the 11th Congress of the Romanian Communist Party (1974), when the *R.C.P. Programme of Creating a Multilaterally Developed Socialist Society and Romania's Path Towards Communism* (published as a separate volume the following year by Editura Politică) had been adopted. This is precisely the moment from which the party-state and its actions in society programmatically (re)define themselves not only in terms of a continuity without breaches with the ancestral history of the nation's body (leaving, therefore, behind the irreducible revolutionary antagonism between the 'past' and the 'new society'), but also in a discourse within which *nation-building* and *state-making* are hard to distinguish. See in this sense the '18 pages of historical-patriotic-nationalist introduction' to the party's programme (Georgescu 1992: 292). Until its abolition in 1989, this programme remained unchanged. Regarding the way the Maramureş region has been integrated into this significant change in the ideological programme of the party, Claude Karnoouh says that 'the global history of the *nemeş* [nobles] from Maramureş has been recuperated by the Communists after 1965–8, as a positive factor, favouring the Romanian peasantry from Maramureş … Later on, the party had been interested in creating an opposition to the Hungarian majority from the local party structures by appealing to the Romanian elites,

[50] Verdery: 'the discourse on the Nation created an ongoing potential for opposition to the activities of those in power, as it entrenched intellectuals within partly autonomous institutions from which they could argue against the direction of state-making' (1990: 83).

who were basically the *nemeş* [nobles] the party attempted to co-opt.'[51] Otherwise, during the period when the demolishing of churches started in Bucharest (in 1977), 'in Maramureş, with the support of local authorities, Eastern Orthodox churches were renovated and even built in order to oppose Greek-Catholicism'[52] (which, during the period of the Austro-Hungarian empire, had been a factor in preserving Romanian-ness, and after the coming to power of communism appeared as a factor of resistance against the regime). 'In this perspective', notes Claude Karnoouh, 'the establishment of the open-air museum is not incidental, and neither is it incidental that Mihai Pop has been so present in this region. In his turn, Mihai Pop has been the descendant of a great and important family of nobles.'[53]

Consequently, the case of the open-air museum in Sighet (its museological concept included) should be read and analysed not so much in terms of 'dissidence' (or ethnological 'heresy'), but in terms of *negotiation* – in a controlled environment, with rules known by everyone – opening the way to the existence of 'give-and-take' cultural forms and practices, with various levels of institutional formalization. One could launch the hypothesis that not only these forms, but also the practice of negotiation generating them have acquired a structural value in the social system emerging under the mono-party Communist regime, especially after 1974. A project as the one of the museum in Sighet *bet on* the short-circuits between the political and the administrative plan *as if they were structural elements* of the political system[54] – this is what seems to follow from the strategy applied by the founder of the museum, at every stage of his administration, in order to fulfil his goals. From this perspective, the case presented here is not exceptional, but only representative of the described decisional realm and of an entire system of practices and social behaviours forged in the Romanian society before 1989.

(Translated from Romanian by Alexandru Polgár and Harmony Spivey)

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[51] Discussion with Claude Karnoouh, April 2007.

[52] Although many Greek-Catholic priests from Maramureş were part of the local nobles (C. Karnoouh).

[53] *Ibid*.

[54] 'The most striking cultural phenomenon is perhaps the strong come-back of ideology as the main instrument of social development. Consequently, a new type of intellectual appeared: the party activist with diplomas, titles and pretensions, which treats culture as an ordinary domain of administrative life that should be planned, coordinated and led according to the necessities of the ruling class' (Georgescu 1992: 292).

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PART TWO

BULGARIA

Chapter 6
Struggling for New Paradigms: Personalities and Institutions in Bulgarian Ethnography and Folkloristic (1944-1989)

Milena Benovska-Sabkova

Diversification and institutional expansion under socialism (1944–1989)

Ambivalent development: theoretical renovation vs. instrumentation

Like elsewhere in Central and Eastern Europe, in Bulgaria the socialist development of academic ethnography and folkloristic was marked by the striving for continuity with the intellectual predecessors of pre-socialist times. This has been, however, rather a contradictory process, also accompanied by changes, as much significant and ambivalent, and characteristic both of the academic practices and of overall social life. The purpose of this work is to analyse these changes through the prism of the personalities and their impact on the scientific paradigms and institutions. This will be achieved by outlining the profiles of five leading personalities of Bulgarian ethnography and folkloristic from the socialist period. My interest in this subject has been prompted by the conviction that as in the other spheres of social life, it is precisely the outstanding personalities who influence the course of development and (more or less tangibly) dictate the changes. The selection of these personalities is inevitably subjective, although it has in no way been random. It has been determined not only and not so much by the leading position of a certain scholar in the academic hierarchy. What has been taken into consideration primarily is the impact of the personality on the formation of the research paradigms and on the development of the disciplines: mostly theories, opinions, convictions and even delusions. For this reason I do not deal with influential figures, like some of the directors of the Institute of Ethnography and Museum, whose role has rather been political and administrative and whose research contribution does not show innovation (Stojan Romanski, Hristo Gandev, Veselin Hadžinikolov, Delčo Todorov).

I also include my own personal impressions, acquired in the wake of the early 1970s, during my university student years and later on, during my work as a researcher. I have worked in direct contact with two of the presented scholars and have had the chance of being personally acquainted with the other three. My observations have been based, most of all, on the analysis of publications and archive sources, but also on information orally conveyed. In some cases the latter deals with important matters, such as the principles and techniques of field research, for instance. Notwithstanding the existence of published and unpublished lists of questions and programmes for field research, the young junior ethnographers and folklorists fairly often received their professional qualification by oral instruction. Below I shall outline the social and academic profile of five paradigmatic ethnographers and/or folklorists, but before that I shall briefly sketch the context of the political, social and academic changes into which their biographies fit, and their theoretical legacy.

Political Context and Academic Development

The establishment of a new socio-political system after 9 September 1944 brought to Bulgaria, as well as to the other socialist countries (Hann, Sárkány, Skalník 2005: 10) meant an immediate transformation of the local academic institutions, with long-lasting consequences on the scholarly community. It was in 1947 that the Communist Party established complete control over the academic institutions (cf. the timelines in this volume) and their placement under the control of the Bulgarian Communist Party (hereafter BKP). Socialism had been a modernization project and had been marked by the imprint of positivistic philosophy, characteristic of the nineteenth century; it therefore shared its admiration of and faith in the unlimited possibilities of science. The swift processes of considerable expansion and diversification of the academic institutions and their human potential was also accompanied by the elimination of academic autonomy (cf. Živkova 2006). In March 1947, following the Soviet model, a new law was passed concerning the Bulgarian Academy of Sciences (henceforth BAS), which was awarded the status of 'the highest scientific institute in the country … under the administration of the Council of Ministers'. As part of the process of expansion of the BAS, during the same year (1947) the Institute of Ethnography was set up (with director Stojan Romanski, ethnography professor at the Sofia University for many years in the period between the two world wars). Two years later the Institute merged with the Ethnographic Museum, in existence since 1892. The latter acquired the status of a section of the Institute and this situation remains in force to this day. This was a step along the lines of the institutional diversification of the discipline of 'ethnography'.

Considered up till that time to be philological, the discipline joined the group of the 'historical sciences' (again following the Soviet model, and already as 'an auxiliary historical discipline').

During the early years of its existence, in its work the Institute of Ethnography was strictly controlled by the communist leadership of the BAS. At the same time, the Institute worked on tasks, following the violent socio-economic changes and the agenda of society: these included documentation of village culture, disappearing under the waters of the new dams or traditional village architecture, disappearing under the blows of new housing construction. Along with this, the changes taking place in everyday life as a result of the collectivization in the countryside were also investigated. The relatively tolerant policy of the BKP to the minorities in the 1950s produced results in the accumulation of data and scholarly publications, dealing with them (Koev 1955: 443–50).

In the socialist age the discipline also underwent further expansion in a different institutional context. Its lecturing was further expanded, becoming part of the curricula of the five universities opened (as well as in Sofia, in Veliko Tãrnovo, Plovdiv, Shumen and Blagoevgrad also, cf. Bojadžieva 2001: 212) and of several pedagogical colleges. Ethnography was taught within the frameworks of the history curricula, while folklore has a place in the course of philological education. Ethnographic museology flowered under the form of regional ethnographic museums (or collections within the frameworks of the history museums) in all thirty districts of Bulgaria. In this way the discipline also developed vertical structures, because the Institute of Ethnography and Museum at the BAS was providing (and still does to this day) academic guidance, i.e. control of the regional ethnographic museums. The institutional diversification further continued with the establishment in 1973 of the Institute of Folklore at the BAS, initially, as a separate folklore section of the Institute of Ethnography and Museum. After the establishment of the two institutes of the BAS, the university ceased to be the main research institution with research functions in the sphere of ethnography and folkloristic. From 1953 until 1974, the Institute of Ethnography published as its periodical *Izvestija na Etnografskija institut s muzei pri BAN [News of the Institute of Ethnography and Museum at the BAS]*. In 1975, the two institutes of the BAS began publication of the academic journals *Bãlgarska etnografia [Bulgarian Ethnography]* (since 1995 *Bãlgarska etnologia [Bulgarian Ethnology]*) and *Bãlgarski folklor [Bulgarian Folklore]* (cf. also the timelines in the present volume).

The two journals have contributed considerably to the development and expanding horizon of the discipline. The predominant share of the scholarly publications in the sphere of ethnography and folkloristic have

been the work of the two institutes of the BAS again, which has actually been the logical result of the considerable number of research associates, working in them and engaged, almost exclusively, in research.

The first independent Ethnography Chair (later 'ethnology') was established at the History Faculty of Sofia University in 1981–2. Unlike the central European socialist countries, the study of non-European cultures had not developed in Bulgaria, either under socialism or during the period preceding it. In the mean time, physical anthropology was also represented (as it is currently) on the level of an institute within the frameworks of the biological sciences in the BAS, completely independently and with no connection whatever to the Institute of Ethnography and the Institute of Folklore.

In short, realizing their projects in such a framework of disciplines and institutions were the five leading scientists, I shall present. Chronologically, these are Mihail Arnaudov (1878–1978), Hristo Vakarelski (1896–1979), Petãr Dinekov (1910–92), Stojan Genčev (1936–90), and Todor Ivanov Živkov (1938–2004). A common feature of all of them was an emphatic attachment and intellectual loyalty to Ivan D. Šišmanov, the father of academic ethnography since 1889. Mihail Arnaudov was the direct, and, probably, the most outstanding disciple of Šišmanov. A student of Šišmanov, too, Hristo Vakarelski turned to ethnography under the influence and even on the unambiguous insistence of his professor. For his part, Petãr Dinekov did not have the chance of personally communicating with Šišmanov, because of the latter's premature death in 1928. Nevertheless, Dinekov is considered to be a disciple of Šišmanov, describing the latter to be a model and a paradigmatic figure of a scholar and an intellectual (Dinekov 1978: 6).

Despite the fact that they belonged to a much later generation, the two scholars, whose careers developed wholly under socialism, Stojan Genčev and Todor I.Živkov, also shared their admiration of Šišmanov, although they each emphasized and referred to different aspects of his intellectual heritage. That unusual attachment was due to Šišmanov's impeccable reputation as a scholar, as well as to his remarkable historical role (in his capacity of a politician) in the formation of the educational and cultural institutions of Bulgaria at the end of the nineteenth and early twentieth century. Worth noting is the emphatic striving towards continuity, which has been characteristic of the most outstanding representatives of ethnography and folkloristic under socialism, despite the impact of political pressure and self-censure.

While presenting the five scholars, I shall adhere to a certain pattern. I shall dwell in short on the social origin of each one of them, as well as on their status and attitude to political life in the country; I shall follow their careers briefly; I shall lay emphasis on their theoretical views and their

contributions to the evolvement of the new paradigms in the discipline (in both its branches – ethnography and folkloristic).

Mihail Arnaudov

Both Mihail Arnaudov and Hristo Vakarelski were paradigmatic pre-socialist scholars, who continued to work under socialism. What is more, it is precisely their contributions that were the most productive in the ultimate formation of both the theoretical boundaries of the discipline and of its empirical 'skeleton'. Thanks to his long life – Arnaudov lived in two centuries and was considered 'the living connection between the generations, between past and present' (Dinekov 1978 [1972]: 475) – the overwhelming majority of his works were written prior to the Second World War, but he continued to exert considerable impact through his publications (actively published in subsequent editions) under socialism, too, and this is one of the reasons why his name has been included here.

Like his teacher, Šišmanov, M. Arnaudov not only came from the high strata of Bulgarian society of the late nineteenth and early twentieth century, but he also occupied influential public and political posts in the period between the two world wars. His most active involvement in Bulgarian politics was in the period 1934–6 (Aretov 2006: 180; Dimitrov 2006b: 359). On graduation in Slav philology in Sofia, Arnaudov (again following the example of his teacher) widened his knowledge in prestigious European universities; he placed his investigations on ethnography and folklore within the frameworks of philology and, in compliance with that model, he worked equally systematically in the sphere of literary history, too, in all these spheres displaying extraordinary erudition. By the end of his life his personal library had 6,523 units (Georgieva 2006: 342). His productivity in the fields of literary theory, ethnography and folkloristic was remarkable. This cannot be explained solely by the author's long life, insofar as most of his works were written during the first decades of his career. His publications in the sphere of ethnography and folklore outnumber those of all his predecessors, taken together.

Mihail Arnaudov's role and influence on the development of ethnography and folkloristic in Bulgaria under socialism cannot be elucidated without mentioning that he had been the object of serious political reprisals in the early years of socialist development. For his participation as Minister of Education in the last but one 'bourgeois' government, which had a life of just three months in the second half of 1944, Arnaudov was sentenced to life imprisonment (on 1 February 1945) by the 'people's court'; he spent nearly two years in prison (1 February 1945 until 25 December 1946) and received amnesty at the end of 1946 through the active intercession of communist

intellectuals (Dimitrov 2006: 366, 361–70). On his release, Arnaudov was sixty-eight and 'found himself as in a desert: he had no home, no job, no income, no titles, no prospects'; paying rent, his family lived in his own former home, which had been expropriated before that to the benefit of the state and had been half destroyed (*ibid.*: 366, 389). His political rehabilitation in 1954 (*ibid.*), as well as academic recognition (coming later, mostly through the new editions of his works) did not revoke his ambivalent social status under socialism. It seems a plausible hypothesis that the reprisals against outstanding scholars like Arnaudov (from various branches of knowledge) substantially contributed to the operation of the principle of 'delayed punishment' (Ledeneva 1998), typical for socialism. At least this is one of the possible explanations for the conformist line of conduct, adopted by most of the Bulgarian scholars under socialism, including the most famous.

The assessment made of Arnaudov has been that his works 'reflect the highest development of European scientific thinking for their time' (Dinekov 1978: 14) and that he was the emblematic representative of the discipline in the period between the two world wars (Benovska-Sabkova 1988: 7). Under socialism, he was among the few who had 'first-hand' knowledge about anthropological theories and their history, and had been guided by ambition to take part in their development within the Bulgarian context (cf. Arnaudov 1968 [1934]: 11–135). Arnaudov's role was pioneering in a number of respects. He was the first, moreover, for a long period of time – the most convincing example of combining cabinet and fieldwork; during the first half of the twentieth century he published a significant body of personally collected folklore and ethnographic materials. His investigations laid the foundations of the study of the Bulgarian traditions in the sphere of the ritual, proceeding from personal field observations, and in a comparative Balkan and European context (Benovska 1978: 16–27). The avalanche-like growth of the number of publications on this subject in the 1970s and particularly in the 1980s did not reduce the significance of Arnaudov's accomplishment, because later authors lacked his erudition. Some of his contributions in the sphere of the study of Bulgarian rituals paved the way to an approach of self-exoticism and opened up a new page of our national mythology (e.g. the investigation of the *nestinari* – the ritual dance on fire – Arnaudov 1971–2, vol. I). This can be discussed as a manifestation of moderate ('soft') nationalism (on this, cf. Iliev, in this volume). His nationalism finds expression also in the way whereby Arnaudov selected the places for fieldwork: often in territories, inhabited by ethnic Bulgarians, but disputed (including by military means) by Bulgaria and its Balkan neighbours (cf. Lebedova 2006: 111–15). The periphery of national territory continued to be

a preferred site for fieldwork of ethnographers and folklorists during most of the socialist period.

Arnaudov made use of the theoretical tools, characteristic of anthropology until the 1930s (Benovska-Sabkova 1988: 4). He referred to the works of Franz Boas and of A. Van Gennep, but not to those of Bronislav Malinowski. Mihail Arnaudov's intellectual legacy can also be discussed as a curious combination of views, which seems incompatible at first sight. His methodological approach was based on the presumption of the mental unity of humankind, a unity going beyond any social and cultural context, while at the same time many of his texts were dedicated to the task of nation building, stressing the unique characteristics of the Bulgarian nation. Universalism was the result of the impact of the British evolutionists, and of Sir James Frazer, in particular. What is more, Arnaudov adopted from Frazer an interest in magic as an explanatory model with respect to religious phenomena. In his analyses Arnaudov used the same chaotic and arbitrary comparative method as the evolutionists; he resorted to concepts like 'survival' and 'relict' (Benovska 1978: 18–24), which were central to evolutionism. Concepts like 'relict' continued to be in use under socialism, too, including also by authors with Marxist views (cf. for instance Genčev).

Mihail Arnaudov had been one of the first, if not the first author, to uphold the view of folkloristic as an independent discipline, though connected with ethnography. This conception was substantiated in the first publication of his generalizing work *Očerci po bălgarskija folklor [Essays on Bulgarian Folklore]* (Arnaudov 1934: 167), finding clearer expression in the subsequent edition of the same book (Arnaudov 1968). This is the cause, underlying the different attitude to M. Arnaudov on the part of ethnographers and folklorists, struggling to prove the respective importance of their disciplines. It was precisely the representatives of the Institute of Folklore who were active in maintaining interest in the theoretical heritage and in the memory of the scholar. Obviously, his prestige facilitated the legitimization of the independent institutional existence of folkloristic. Together with the literary historians, the folklorists have marked every jubilee celebration of Arnaudov, since his demise in 1978 to date, with conferences, symposia and collections. Against the background of the anaemic interest in self-reflection, characteristic of Bulgarian ethnography (ethnology) and folkloristic, the numerous publications on the intellectual legacy of Arnaudov have been impressive indeed.

A change in the attitude to Arnaudov has been seen with the passage of time. On the occasion of his centenary jubilee in 1978, I published (as a quite young, novice author) an article on the theoretical heritage of the scientist (Benovska 1978). I was commissioned to write the article by Todor

I. Živkov, who advised me to interpret critically Arnaudov's fascination with the 'survivals' and the 'relicts' (an instruction that I failed to follow in full). Arnaudov's later interpreters gradually adopted a 'hagiographic' approach. Indicative is, for instance, the title *Vekovechniyat [The Ever Lasting]* (a reference to Arnaudov) of the preface to the latest edition of *Ocherci po bãlgarskija folklor [Essays on Bulgarian Folklore]*. The difference in the attitude of literary historians is obvious; they give the scholar his due, without losing the critical distance (cf. for instance Aretov 2006).

Hristo Vakarelski

Vakarelski's village origin and his 'personal profile' closely matches that of the 'double insider', a feature typical of the Balkan ethnologists, according to Slobodan Naumović (1998: 102). Vakarelski's youth was characterized by privations, while the historical trials and tribulations were part of his personal biography: he took part in the two world wars as acting officer. His activity was almost equally long before and under socialism. Vakarelski was the first researcher, to fully dedicate himself to ethnography, without combining it with work in the field of philology.

Hristo Vakarelski's academic career started relatively late, when he was thirty, on the insistence of Ivan D. Šišmanov, who saw in him the future successful ethnographer; he also became his 'spiritual grandson' (Vakarelski 2002: 52–3). The situation was almost comical. While a university student, Vakarelski's conception of ethnography had been as an abstract occupation with the history of the Slav peoples. For that matter he met without any enthusiasm the possibility of specializing in Poland in ethnography (*ibid.*: 36–42). However, that specialization in Poland, reluctantly accepted (1926–7), during which he attended lectures by Eugenius Frankowski and Stanislaw Poniatowski, completely altered the life and views of this humble country teacher. His dislike of ethnography rapidly turned into a tireless interest and dedication to the profession. In 1927 he started work at the Ethnographic Museum (later on, when it merged with the Institute, at the Institute of Ethnography and Museum). He continued working there until his retirement in 1962, with an interruption in 1941–4, when he was Director of the Popular Museum in Skopje, in the territories of the present-day Republic of Macedonia, at that time occupied by German and Bulgarian troops.

Hristo Vakarelski was remarkably productive. His bibliography includes about 950 books, studies and articles (cf. Krãstanova 1979: 23). He was the paradigmatic, 'most striking representative [of Bulgarian ethnography] in the course of almost fifty years' (Stojkova 1976: 16; cf. also Genčev 1990: 5). His work covered practically the entire empirical range of Bulgarian traditional culture: from material culture to folklore music. His interest in

material culture, however, predominated (about 270 of his publications were devoted to it, cf. Genčev 1990: 5). This was also a reaction to the disproportionate interest in songs, legends, rites and rituals that predominated up to the 1930s. The emphasis on material culture was also probably associated with the influence of Polish ethnography in the period between the two world wars.

Fieldwork was one of Hristo Vakarelski's strong points. He practised comparatively longer sojourns in the field (for two or three months – Vakarelski 2002), unlike the later practice of the 1970s and 1980s. In the years preceding the Second World War, he carried out fieldwork individually, while in the socialist period he took part in collective 'complex expeditions', sometimes consisting of as many as thirty people (Ognjanova 2002: 13). At the same time 'salvage philosophy' (enduring for decades in Bulgaria) was his main motivation in field research. The frank empiricism of part of his work has also to be understood in this way. This also determined the trend to archaism (*viz*. the search for and construction of 'the oldest strata' in culture), which was characteristic of the methodological instructions and questionnaires composed by Vakarelski for documenting rural culture (Vakarelski 1946). And despite this, the scholar also took an interest in the changes, occurring in everyday life in the conditions of socialist modernization (cf. for instance Wakarelski 1956: 365–71).

According to Hristo Vakarelski, ethnography is a science 'about the cultural history of the working population' (Vakarelski 2002). Unlike M. Arnaudov, he developed the thesis (formulated in a completely non-conflicting way) of folkloristic as part of the broader discipline of ethnography (cf. Vakarelski 1974). His folkloristic contributions, making up quite a considerable share of his publications, should also be construed in this way (cf. Stojkova 1979: 16–22). Since his death this thesis has been used as a legitimization argument (Todorov) in the debates of ethnographers with folklorists (about which, see Garnizov in this volume).

In comparison with the older Arnaudov, Vakarelski was less interested in the development of theories in anthropology. The traces of long standing/ *longue durée* of evolutionism, however, were visible here, too, mostly in the concepts and interpretative models used. Ethnocentrism was not alien to Vakarelski. Parallel to that, the scholar placed Bulgarian culture within the context of the most general patterns of the development of civilization (no matter whether we now accept them as trustworthy or not). In his most serious works he applied the comparative approach, most often on Balkan and/or Slav basis (Primovski 1979: 12; cf. Genčev 1990: 7), but he also had articles establishing closeness between the Bulgarian and Hungarian traditional culture (Wakarelski 1965a: 367–73).

The 'patriotic' tendencies in Vakarelski did not interfere in any way with his maintaining international contacts of remarkable endurance. His correspondence is evidence of this: out of about 9,000 letters (by him and to him), 4,000 are to or from foreign scholars from almost all the European countries (cf. Primakovski 1979: 14; Vakarelska 1984: 67–78). He helped a great number of his colleagues by providing information, by accompanying them in field research in Bulgaria. At the same time he never ceased learning from them, particularly with regard to fieldwork. Quite often incentives to write some work 'from the outside' came from foreign colleagues (including also for *Etnografia na Bălgarija [Ethnography of Bulgaria]* – Primakovski 1979: 14). For his *Za prinos kum koultournoto sutroudnichestvo mezhdou narodite v Evropa [For a Contribution to the Cultural Cooperation Among the Peoples in Europe]* he received the Herder Award in 1965 (*ibid*.: 15).

Vakarelski had no political ambitions and was not a member of any party, even under socialism. In that period he did not particularly alter his research views. This was probably why he found it quite difficult to adapt to 'Marxist methodology' (Dinekov 1978: 438–9). To put it more clearly, he did not stay away from the atmosphere of conflict between the opposing factions, which so much overwhelmed the Institute of Ethnography and Museum, that for a certain period, the following slogan was placed in its building: 'Let us forget all the personal squabbles and dedicate ourselves to scientific work!' (Ognjanova 2002: 7). It is hard to say whether the contradictions between the non-party people and the BKP members had found expression in personal conflicts, or the party members had abused their political resource to exercise personal clout. Vakarelski's memoirs leave the impression that the latter had rather been the case. Notwithstanding all this, Hristo Vakarelski has received the recognition due to him: his jubilees were twice marked, the second – in connection with his eightieth birthday; two volumes of studies have been published in his honour (Bogdanova *et al*. 1979; Hadžinikolov *et al*. 1980).

Petăr Dinekov

Dinekov was the founding director of the Institute of Folklore from 1973 to 1983 and editor-in-chief of the *Bălgarski folklor [Bulgarian Folklore]* journal over the same period. When as a first-year university student I first met him in 1971, he was among the few representatives of the outgoing generation of pre-war erudite scientists in Bulgarian academic circles. The university courses in folklore used his voluminous book *Bălgarski folklor [Bulgarian Folklore]* (1959), and we students tried to skip the boring Marxist clichés of an obviously 'decorative' character, which were to be found in it. I have never heard him use these clichés in a conversation. Rumours

assigned him the reputation of 'a bourgeois scholar', while his old-fashioned refined manners seemed to confirm this. His academic career began on the eve of the Second World War, but most of it developed under socialism. A non-party member, Dinekov was closest to the model of the Western-type scholar and intellectual (in the terms of Max Weber), completely dedicated to science.

Though born in the country, in a modest family, he was 'a child of the town' (Nikolova 2001: 7), spending his whole life in the capital; in his maturity he was at the core of the country's elite of writers and scholars. He received his higher education thanks to the Talented Children Fund (he was never to forget the help received and later 'reimbursed' it many times over through his selfless and generous help towards a considerable number of capable university students). Graduating from the university in 1933, he already had more than 200 publications in the Bulgarian periodical press. He specialized in Poland (1934–5) and made an unusually successful university career: in 1938 he was assistant professor, and as early as 1945 he was elected professor at the St Kliment Ohridski University of Sofia. Dinekov was for many years editor-in-chief of several prestigious Bulgarian academic publications: *Spisanie na BAN [Journal of the BAS]* (1957–75) and *Bãlgarski folklor [Bulgarian Folklore]* (1975–83), the series *Problemi na bãlgarskiya folklor [Problems of Bulgarian Folklore]* (1972–87), *Starobãlgarska literatura [Old Bulgarian Literature]* (1971–92), *Kirilo-metodievski studii [Studies on Cyril and Methodius]* (1984–92) (*ibid*.: 8–10, 12).

Dinekov's contributions to folkloristic were just a part of his extensive activities: he was also a literary historian, literary critic, translator, editor, author and co-author of a great number of textbooks. He has mostly been identified as a literary historian (Dinekov 1978: 6), and of the five scholars presented here he was closest to the philological paradigm, established by I. D. Šišmanov. Dinekov upheld the conception of folklore as 'oral literature' (familiar also in other Slav countries). In conformity with this, folkloristic was, in his view, connected with ethnography without being a part of it and should develop as an independent discipline, applying philological methods (cf. Dinekov 1990: 5).

His research contributions to folkloristic were focused in his book *Bãlgarski folklor [Bulgarian Folklore]*, in a great number of articles, in work on the critical publication and subsequent editions of collections and anthologies of folklore. Of particular note are his contributions to the sphere of the history of folkloristic and to the study of the problem of interaction between folklore and literature. Accused of being outdated (Holevič 2001), Dinekov was actually characterized as reflexive, as constantly thinking about strategy and as responsible for the development of the discipline. What can

be found in his articles is an attention to the tendencies in the development of folkloristic and a programme of its topical tasks (Dinekov 1972: 13–15; 1975: 1–9); he was also the author of the only comprehensive historical review of the development of folkloristic in Bulgaria so far (a chapter of the book *Bãlgarski folklor [Bulgarian Folklore]* – Dinekov 1990: 71–164).

His contributions to the institutional expansion and strengthening of the discipline were significant. In 1973 he backed the wishes of six members of the folklore section at the Institute of Ethnography to separate and to establish a new research unit, breaking away from self-sufficient empiricism. They had been the more active part of the Institute of Ethnography in the development of projects and in issuing publications and they wished to develop a new research paradigm. Dinekov helped them with his reputation and prestige not only in academic circles, but also in society as a whole. For example: the decision to launch the new *Bãlgarski folklor [Bulgarian Folklore]* journal was taken literally within minutes after a statement by Dinekov at a joint session of the Academic Board of the Sofia University and the Presidium of the BAS in 1974. I attended that session as a representative of the university students and I can testify that the decision was made without any problem, due to Dinekov's undisputed prestige in both institutions.

The structure of the new institute, however, followed the conceptions of one of the six – Todor I. Živkov (see below), rather than Dinekov, and this is evidence of the tolerance, openness to new ideas, and the spirit of cooperation cultivated by Dinekov. Under his leadership, the Institute of Folklore developed a number of new projects (the Catalogue of Bulgarian Tales was one among them) and new publications. Ever since 1971, symposia on Problems of Bulgarian Folklore have been organized (the papers are published in a series of the same title), with significant international participation. Broad international contacts have been maintained – quite a few of them with folklorists and ethnographers fromsocialist countries, but also from a number of west European countries and the USA. When this practice is compared with the policy of isolationism pursued by Veselin Hadžinikolov, at that time Director of the Institute of Ethnography and Museum (Mihajlova 2006: 13), this leads to various ways of thinking. It was yet more evidence of the influence of the leading personalities on the work of the institutions; on the other hand, we are also bound to discuss the flexibility of political control during the different periods of socialism.

The two institutes shared the same building up until 1988. Dinekov's personality and contribution were reflected by the fact that during the early years of his work as director, he had no office and set aside a considerable share of his salary for the upkeep of certain functions of the institute, for which no financing had been provided. Official meetings with him fairly

often took place in his home, where the associates were received as guests. Against this background, it is understandable why his sudden release from the director's post in 1983 by a resolution of the Central Committee of the BKP and without any explanation whatever, was traumatic to him.

Dinekov was indeed an exemplary personality and a moral model for those who knew him. That is why 'a significant and prestigious school' took shape around him (Nikolova 2001: 15). In his own view, his most important achievement was the formation of the young researchers (Dinekov 1978: 6–7). That is why his role exceeded the direct importance of his works.

Stojan Genčev

The 1970s were marked by the presence of a new generation of researchers with ambitions to reinterpret their intellectual heritage and to map out new roads for the development of the disciplines (Bojadžieva 2001: 215). Standing out prominently among them have been without doubt Stojan Genčev and Todor I.Živkov.

Associated with the name of Stojan Genčev were the most serious ambitions for the theoretical renewal of Bulgarian ethnography in the 1970s and 1980s. He was born in a village environment, and graduated in philology in 1961. Shortly afterwards, having defended his Ph.D. thesis at the Institute of Ethnography and Museum in 1970, Genčev already had the reputation as a leading researcher at the Institute (Kalojanov 1996: 395), occupying positions in its internal hierarchy and initiating some of its most significant projects and publications. He was in charge of the section of Traditional Spiritual and Socio-Regulative Culture over a long period of time; he worked as a scientific secretary, deputy director, and for a short period in 1990, director of the Institute. He was editor-in-chief of a number of the collective publications of the Institute of Ethnography in the 1970s and 1980s, and the initiator of the series *Etnografski problemi na narodnata kultura* (*Ethnographic Problems of Popular Culture*, 1989); he also lectured at the universities in Sofia and Veliko Turnovo.

In his short life Stojan Genčev published two books and about 130 articles and studies (Genčeva 1991: 9–14; Popov 1996: 5–6). Some of them were devoted to Bulgarian rituals (sometimes analysed comparatively). This subject was among the most extensively developed in the 1970s and 1980s and engaged the efforts of at least two dozen researchers from the institutes of the BAS and from the universities, whereby the field research techniques of the retrospective interview were applied, by means of which practices from the early half of the twentieth century were reconstructed. One of the most frequently quoted works of Stojan Genčev (1973a: 89–112; 1974b: 83–109) from this thematic range is his study on the custom of *kumstvo* (the

custom of becoming witness at a marriage, and subsequently – godfather of the children born) as a social phenomenon and a form of ritual kinship.

S. Genčev's book *Narodna kultura i etnografija* (*Popular Culture and Ethnography*, 1984) is completely theoretical. Elaborated in it was the conception of 'popular culture' as a system and as a research object of ethnography. Written in the 1970s and published later, the book filled a void after three decades of theoretical fragmentation, when the foundations and the boundaries of the discipline had been considered a fact. It has also been a 'retort' to Jurij Bromlej's theory of ethnos (1976). Although a substantial part of it was devoted to the theory of the ethnic (cf. Bojadžieva 2001: 215), the emphasis falls elsewhere: on the characteristics, functions and chronological boundaries of popular culture, its historical development and its present-day transformations. The conception of ethnography as a discipline of popular culture aims at continuing the Bulgarian intellectual tradition, but also at expressing a view, which has to remain 'Marxist', and to be, at the same time, independent of Soviet theories. Actually, the two indubitably leading figures of ethnography and folkloristic – S. Genčev and T. I. Živkov – adopted as their first-rate obligation (as well as a privilege) to write on the problems of the theory of ethnos. The two were in an indirect dialogue. *Narodna kultura i etnografija [Popular Culture and Ethnography]* by Genčev can also be construed as 'a retort' to the theory of folklore, contained in *Narod i Pessen [People and Song]* by Živkov (1977); somewhat later an 'response' by default was given by Živkov (1987: 55–128; 1994) to Genčev on the questions of ethnos.

The principles and techniques of fieldwork, in the way they were established in Bulgaria under socialism, were subjected to discussion in Stojan Genčev's other book *Terenno etnografsko izsledvane* (*Ethnographic Field Research*, 1990; see also Asen Balikci's chapter in this volume). In it the author summed up his own, as well as the collective experience, in empirical fieldwork. The book has both theoretical and practical significance. It discusses the importance of fieldwork as a process, in which ethnography creates its own sources of investigation. The search for trustworthiness and verification of the collected material shows the importance of precision and responsibility during fieldwork. At the same time the book is also a testimony to the techniques, adopted under socialism, pride of place among which occupied the quasi-structured enquiry. The series of questionnaires enclosed, accompanied with diagrams and sketches have been a useful aid, particularly for novice researchers (cf. Popov 1996: 8).

Different parts of Stojan Genčev's theoretical heritage were variously received during the 1990s. His characteristic of traditional ('popular') culture has quite a number of supporters among present-day ethnologists (i.e.

disciples of ethnography), whereas the theories about ethnos have gradually been abandoned. Although one could hardly speak about a 'school', Stojan Genčev has disciples and followers today (one of them is Račko Popov, the current director of the Institute of Ethnography and Museum), who publish new editions of his works (Genčev 1996) and keep his memory alive through conferences and books, dedicated to him.

Todor Ivanov Živkov

Todor Ivanov Živkov was the second director of the Institute of Folklore – from 1983 until 1992 (i.e. until the law on the decommunization of science came into force. About that, cf. Garnizov in this volume). He was the editor-in-chief of the *Bălgarski folklor [Bulgarian Folklore]* journal from 1983 until his death in 2001. The theoretical renovation in the 1970s was largely associated with his name. He was the ambitious young man, whose striving and search for new paradigms of investigation changed not only folkloristic in the 1970s, but to some extent ethnography, as well, which had to respond to the challenge addressed to it. T. I. Živkov has been discussed in great detail in this volume by one of his closest disciples, Vassil Garnizov, who still remains loyal in many respects to his former mentor (cf. Garnizov's chapter in this volume). For that reason I shall outline just the basic aspects of his ideas and his activities.

Todor I. Živkov was of village origin, but he had an ambivalent attitude to the countryside: he did not identify with his origins, insofar as he attained a high social status and made a successful political career (for many years BKP Party Secretary of the BAS, instructor at the Central Committee of the BKP and, during the second half of the 1980s, personal advisor to the head of state; national representative (parliament deputy) of the Bulgarian Socialist Party 1994–7).

Živkov was one of the six founders of the Institute of Folklore in 1973. In that he cooperated with Petăr Dinekov and played an important, though not so obvious role. According to the 1947 law, the BAS was under the direct administration of the Council of Ministers (see above), while the establishment of scientific institutes quite often took place in Bulgaria with the direct blessing and intervention of the head of state (cf. Čalăkov 1998: 18). For that reason, Živkov's stand was of essential importance. With his prestige, Petăr Dinekov publicly justified the need for such an institution but it can be assumed that Zhivkov contributed to ensuring party backing.

In 1977 he published his work *Narod i Pessen [People and Song]*. The book was extraordinarily innovative, and stirred up a powerful and long-term effect, not only among folklorists, but among broader circles of the social and humanitarian disciplines in Bulgaria. This was not just the next

new conception, but the first Bulgarian attempt at defining folklore for itself, rather than through the broader or closer differentiation from traditional culture (Arnaudov) or from literature (Dinekov). Živkov built this theory under the impact of the socialist conceptions about social communities and groups, as well as of Marxist understanding about 'unalienated labour'. The idea of folklore was developed in *People and Song* as 'a type of artistic culture' as a kind of superstructure, regulating the relationships between the individual and society. Folklore was a dominant cultural expression in the societies, based on 'unalienated labour', whereas the main social field, where this artistic culture functioned was the settlement, the community. Folklore was also an integral system of each society, insofar as it was designed to meet the basic human need of creative activity (Živkov 1977: 11–13, 25–6). It is not hard to find a certain closeness between Arnaudov's theory of folklore as 'spiritual culture' and the quoted view regarding 'popular artistic culture'. There are three basic spheres of 'popular artistic culture': songs, tales and plastic folklore/folk dancing (*ibid*.: 33–5).

This theoretical model was embedded in the structure of the newly established Institute of Folklore. Initially, its sections were 'Oral Folklore', 'Theory and Aesthetics of Folklore' and 'Plastic Folklore/Folk Dancing' (cf. Dinekov 1978), while since 1985 the sections of 'Music Folklore' and 'Slav and Balkan Folklore' have been added. This is indicative of Živkov's impact on the theoretical and institutional development of folkloristic. As I mentioned earlier, ethnography also felt, though indirectly, the influence of Živkov's work. The powerful theoretical 'emphasis' of his work, particularly through and since *People and Song*, has shown the untenability of, and has put an end to, the self-sufficient empiricism, dominant for about two decades.

The conception of folklore as 'popular artistic culture' evolved (cf. Garnizov, in this volume) in Živkov's subsequent books and articles (1987). On the one hand, that evolution was an expression of the constant concern, so characteristic of him, to improve the theory. In this way the formulation of 'popular artistic culture' has been substituted by the more vague and somewhat tautological concept of 'folklore culture', which also reveals the author's ambitions for 'rights' on the common field of ethnography and folkloristic, i.e. on the research of a broader object sphere. At the same time, the expansion of the range (and ambitions) of folkloristic blurred its specificity and autonomy with respect to ethnography. The different attempts in the wake of 1989 at renaming ethnography (to ethnology, cf. Ivanova 1995) and of folkloristic, first as ethnology (cf. Raičeva 1997; Bojadzhieva 2001), and after that – also as anthropology (Bojadžieva 2001; cf. Elčinova 2006) have since illustrated the watering down of the boundaries between the two disci-

plines. Živkov was among the first in the action associated with the campaign to rename the Turks in the 1990s (cf. Živkov 1994: 3–11; 2000a).

In 1976 T. I. Živkov specialized for six months in France at the Claude Levi-Strauss Laboratory in Paris, coming back full of ambitious plans and the desire to keep a debate going. His publications do not reveal any influence of Levi-Strauss or of Roland Barthes, but Živkov did encourage his associates to develop semiotic or structuralistic approaches and methods (cf. Garnizov in this volume). As a witness of, and participant in, what was taking place at that time, I can say that following this fashionable methodology did not require any special heroism. Our self-hermeticism in the sphere of mythological reconstructions was a form of escapism, as well as a desire to stay away from the cultural engineering, characteristic of the decade from the mid 1970s until the early 1980s. This form of instrumentation (voluntary or coercive) was linked with the attempts to enforce 'the socialist holidays and rituals' as a form of 'struggle against religious prejudices', in which a group of party affiliated ethnographers were particularly active and from which Živkov did not manage to stay away completely (cf. Petrov 1997). Semiotics was an alternative and a further more serious attempt (not realized, due to the collapse of the regime in 1989) at the political use of ethnography and folkloristic in 1985–9, when the campaign of coercive name-changing of the Bulgarian Turks was carried through. Barring that period, the academic climate during the 1970s and 1980s was generally characterized by relative tolerance (cf. also Popov 2003: 7–12).

Though contradictory as a personality, Todor I. Živkov attracted a great number of followers among the young (he liked lecturing, though he lectured full time only after 1990), whilst up until 1989 he practised it mostly through the Ivan D. Šišmanov Study Group of Folklore (cf. Garnizov in this volume). In this way he created a circle, a group of followers and adherents, leaving a lasting imprint on their scientific views.

Živkov's scientific legacy has shown that the scientific occupations under socialism cannot be reduced solely to the simplified pattern 'an honest non-party intellectual' vs. 'a party activist, making use of the political resources to advance his scientific career'. This alternative did exist under socialism, but it was not generally valid for the scholarly life of that period, either.

Conclusion

The concise biographical data about the five researchers presented make it possible to outline their social profile, which has undoubtedly left an imprint on their theoretical constructions. Matching the type of 'double insider' (according to Naumović) – researcher and moral proponent of rural culture,

himself of rural stock, was Hristo Vakarelski. Mihail Arnaudov was close to the West European type of intellectual, coming from and belonging to the high strata of society. His active involvement in politics prior to 1944, however, also brings him closer to the East European tradition of combining a career in science with a political career. The remaining three did not identify themselves with the countryside, despite their rural origin. In fact their biographies followed a trajectory, typical of the life of millions of people under socialism: migration from the country to the town.

Arnaudov, Vakarelski and Dinekov pursued their careers in two ages (up to, and under socialism) and adapted with varying degree of success to the conditions under socialism. The political reprisals against Arnaudov in the 1940s and 1950s were, in my view, a signal for his followers of the imperative need for a precise strategy of adaptation to the system. At the same time, the three of them carried through the continuity between the founding fathers of the discipline in Bulgaria from the end of the nineteenth century and the new generations of researchers, born and having grown up under socialism. Genčev and Živkov worked entirely under socialism, but also shared the striving for continuity. They, in turn, have also left a significant imprint on the postsocialist development of ethnography and folkloristic. The biographies of the five of them illustrate to what a significant degree personalities can determine the image of institutional development.

Under socialism, working on ethnography and folkloristic became a profession. When it was established in 1947, the Institute of Ethnography had three research fellows, whereas by the end of the socialist period, about forty-five researchers worked there, and about thirty researchers at the Institute of Folklore (excluding the museum workers and the rest of the staff). At least two dozen scholars lectured at the universities, whereas the number of museum workers throughout the country has not been specified, but it is a safe bet that there were no less than several dozen. This entire cohort was involved in various ways and to a different extent in the 'production of knowledge', but at the final count this has been a fruitful process, yielding an impressive flow of publications. Ethnographers and folklorists are social actors and mediators in yet another process – the transformation of rural culture from a living reality into a cultural heritage.

Throughout the entire socialist period, however, the discipline developed under the impact of contradictory trends: between the dominating striving for continuity on the one hand, and the breaches of political pressure and the manifestations of instrumentation, on the other. In this sense these intellectual trajectories have likewise been an expression of the contradictory nature of socialism as a whole. The evolutionist theoretical models have proved long lasting in Bulgaria under socialism, and Marxism, with the

'petrified' evolutionism contained in it, was particularly instrumental. Along with this, new research paradigms have developed, lending a specific appearance to Bulgarian ethnography and folkloristic, although no clear-cut boundary between the two can be delineated. The significant intellectual and human potential, created under socialism, has been a resource for the search for new ways and new theoretical horizons. But this will be the subject of another article.

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Chapter 7
Local Uses of Three Soviet Ethnographic Concepts

Ilia Iliev

A major characteristic of Bulgarian ethnology is that from its very beginning in the early nineteenth century it was strongly influenced by its Russian counterpart. Academic institutions, several analytical tools and preoccupations were largely shaped after the Russian model during most of the nineteenth and twentieth centuries. The socialist period was but one of the peaks of this long-term trend. However, I will argue that both the academic institutions and the intellectual tools were creatively adapted to local needs, while preserving most of the formal characteristics of the initial model. They provided Bulgarian scholars during the socialist period with alternatives acceptable to the ruling Communist Party (BKP), which nonetheless conformed to the not always coherent policies of the totalitarian state, and allowed for choices, internal debates and heated arguments.

A Strong Russian Influence

The first texts on Bulgarian folklore were written in the early nineteenth century by the Russian subject Jurij Venelin, a Slavophile enthusiast who became interested in the culture of the then virtually unknown Slavic nation beyond the Danube, and whose works inspired the first Bulgarian ethnographers.[1] Learned men from other Slavic nations became interested in Bulgarian popular culture later, and Bulgarian students trained in Central Europe played a major role in the development of the discipline (Valchinova 2004: 3), alongside graduates from Russian universities, helping the nascent Bulgarian *etnografija* reach an honourable position amongst the central and east European ethnologies. However, the Russian influence remained dominant, due in large degree to the specific development of the Bulgarian language. The modern Bulgarian language was constructed in the nineteenth century through a process of standardizing the local dialects, and by cleansing the words of presumably non-Bulgarian (mostly Turkish) origin. The process

[1] For the beginning and early history of Bulgarian ethnology, see Todorov 1989.

was marked by massive borrowing from Russian. Several professional domains adopted their terminology directly from Russian, e.g. the state administration, the army, and education. Poets and writers helped introduce new Russian words into everyday life, while the Bulgarian Orthodox Church kept using Church Slavonic – the same as in the Russian Orthodox Church, which gladly lent church books to the Bulgarian priests during the whole Ottoman period. As a rule, taking words from Russian was not considered borrowing from a foreign language. On the contrary, it was usually seen as a return to the true identity of the Bulgarian language, which had lost some of its purity under the Ottoman empire.

During the socialist period, the social sciences continued this tradition of direct borrowing of concepts, terms and analytical tools from Russian, often skipping the process of intellectual translation, without debates about the proper meaning of the tool, about its relations with other analytical concepts, or the nuances and connotations it implied. The language was not a screen protecting local intellectual traditions; it facilitated the transmission of Soviet influence. Ethnology was one of the disciplines where Russian influence was felt the strongest before 1944. It had been taught at the Department of Slavic Languages and Ethnography since 1891, and its focus was on comparative Slavic ethnography. The professors regularly stressed the common cultural heritage of the Slavic nations, including Bulgarians and Russians. During the 1930s and early 1940s, when Bulgaria was an ally of the Third Reich, the official propaganda argued that Bulgarian ancestry was mostly Turkic or Hun, 'the Arians of the East', while the professors of Slavic ethnography kept insisting on the common Slavic heritage and enjoyed a reputation for being Slavophiles. Maybe this could explain why the transition after 1944 was relatively smooth, and Prof. Stojan Romanski, who was head of the Department of Slavic Languages and Ethnography at the University of Sofia before 1944, became director of the newly established Institute of Ethnography. Milena Benovska-Sabkova's chapter in this volume presents several examples of leading ethnographers or folklorists, who continued their career after 1944.[2] Besides the relatively smooth transition, Bulgarian ethnologists also enjoyed the power of choice. Soviet ethnography was far from homogenous, and several trends, analytical tools and styles of working and writing were available. The policy of the Bulgarian socialist state was not monolithic either, so the ethnographers could also choose between different priorities here. This allowed for relative freedom of choice, for internal debates within the field of Bulgarian ethnography, and for the emerging of different trends. I will mention three key concepts, 'bit',

[2] Regarding the dramatic changes in other social sciences, see Živkova 2006.

'ethnos', and 'way of living', which played an important role in Bulgarian ethnography at different periods of time, and will briefly discuss their relations with state policy.

Intellectual Tools: Bit

The word 'bit' was borrowed in the modern Bulgarian language from Russian, where it had an impressive intellectual history. Svetlana Boym argues that the opposition between bit, everyday existence (everyday routine and stagnation), and bitie (spiritual being), is one of the central commonplaces of the Russian intellectual tradition, and is often understood as the opposition between everyday life and 'real', spiritual life (Boym 1994: 29) The first book of the Bible, Genesis, is translated into Church Slavonic as 'Bitie', while bit denoted humble everyday activities. Such a distinction, bit versus bitie, was lacking in the modern Bulgarian language. The first Bulgarian dictionary, published at the end of the nineteenth century, does not mention 'bit'. Actually, such a word existed: 'bit' in Turkish signified 'flea' and the root is still productive in the modern Bulgarian language – e.g. bitpasar means flea-market. As with many other Turkish words, this one was quietly dropped from the modern Bulgarian language, and a Russian word was introduced instead. But it was a direct transfer, bypassing all the complexities of intellectual translation: Bulgarian authors just began using the word as self-evident. In the first half of the twentieth century, the term 'bit' was sporadically used by Bulgarian ethnographers and sociologists much in the same way as in pre-war Russian ethnography, where it was a loose equivalent of the English terms 'daily life', 'domesticity', 'lifestyle' or 'way of life', and was used to describe differences in daily life represented by traditional ethnographic subjects, such as Siberian aboriginal groups, as well as traditional European peasant societies (Buchli 1999: 23).

Meanwhile, in the modern Russian language the concept of bit acquired new significance. After the Revolution, the term began to denote all the material and cultural conditions necessary for the social reproduction of the workforce and human beings in general. In a much quoted text Anatolij Lunačarskij drew a clear distinction between the domain of 'bit' and the domains of socialist production and socialist politics (Lunačarskij 1927). His definition remained practically unchallenged during most of the whole socialist period. The *Encyclopedic Dictionary of Philosophy* from 1983, and edition of the Soviet Academy of Sciences, defines 'bit' as 'a domain of social life outside production, which includes the satisfaction of the human needs of food, clothing, housing, medical aid, as well as the consumption of cultural goods, communication, and rest' (Frolov and Harchev 1983: 69; see

also Bromlej 1976: 50; for an earlier Soviet definition, closer to the pre-revolutionary meaning, see Stepanov-Skvorcovski 1927).

The social sciences in socialist Bulgaria gladly adopted the term of bit, but in a relatively simplified way. Ethnologists developed the concept in strong cooperation with philosophers, sociologists and propagandists, as part of a larger effort to modernize Bulgarian society.[3] In Bulgarian texts from 1950s and 1960s, 'bit' signifies mostly the material conditions of everyday life, and is opposed to more spiritual things and practices.

For example the philosopher Nikolaj Mizov defines bit as 'way of eating and dressing, interior design of the house, personal hygiene and hygiene in public spaces, personal relations, especially within the family' (Mizov 1965: 5). The ethnologist Rajna Peševa plainly contrasts 'bit' with 'ideas', and claims that 'several times the bit prevents introduction of progressive ideas' (Peševa 1963b: 5). In 1965 Professor Geno Conkov, a professional propagandist, offered a vivid and detailed account of how the bit might influence the socialist citizen. There are people, he wrote, who are obsessed with things, and for whom acquiring things becomes a primary aim. He gives a list of such things: 'more and better pieces of furniture, clothing, shoes, books, radio and TV, photo camera, tape recorder, cooking ovens, washing machine, vacuum cleanser, motorcycle, private car, country house, etc.' (Conkov 1965: 48). They buy things, material things for everyday use, which represent the core of the bit. And the bit starts changing human beings, people become eager for more and more new acquisitions. The next step is to change your own body, by 'primitivism and extravagance in clothing and hairstyle' (Conkov 1965: 51). The next step is a deterioration of interpersonal relationships within the family, men who 'despise women's work', and live parasitically on their wives' work, who 'spend their time in pubs and restaurants, and playing cards and betting'. Privileging the domain of bit, the individual inevitably starts neglecting public domains. He misuses public property, 'tramways and trolley buses, telephone posts, street lights, garden and park lights', etc. Finally, the individual, influenced by the bit, starts neglecting the public values and begins to listen to foreign radio stations, explains Conkov. Everyday life, and especially its material side, was interpreted as specific kind of infrastructure, able to shape the consciousness of the individuals.[4]

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[3] Scholars of different academic and institutional backgrounds took part in these efforts, and as a rule, the ethnologists did not play the most important part in these teams. Discussing the official policy of the state, I would prefer to quote those occupying the most prominent institutional positions, instead of the most intellectually stimulating ones.

[4] Julian Bromlej refers to Geno Conkov in his discussion of the concept of bit; see Bromlej 1976: 74, n. 11.

Classifying Social Groups: Bit and Class Hierarchies

The new socialist state had ambitious projects for modernizing the Bulgarian state, economy and society.[5] It happened that the social groups who were considered especially backward and therefore deserving the direct attention of the state, were not those identified as class enemies. The peasants undergoing a process of collectivization, the inhabitants of isolated mountain areas, the workers coming to the newly built socialist cities from the countryside, the ethnic minorities, the housewives who had to be recruited as workforce – all these social groups and categories had to be subjected to the socialist civilizing process, and the domain of bit obviously required a stricter regulation from the socialist state. The study of bit was declared a priority of the Institute of Ethnography,[6] and several researchers were directed towards investigation of the bit of the working class, the few already

[5] Regarding the modernization projects of the Bulgarian socialist state, see the outstanding work of Brunnbauer 2007, especially 221–364 and 475–605.

[6] Bulgarian ethnology has an impressive record in facilitating different modernization efforts. Several nineteenth-century ethnographers were also the most prominent engineers and ideologists of the Bulgarian nation, and did not see a major dilemma between the national traditions and the modernization of society. This is largely due to the image of the Bulgarian nation they tried to share. In their views, the very essence of the Bulgarian nation was European, it was a sober and pragmatic nation, victim of a particularly adverse set of circumstance, which had fallen under the combined rule of two Asian tribes – the Turks and the Byzantines, controlling the Orthodox church (our south-western neighbours were usually referred to as Byzantines, implying their Asiatic belonging, more rarely as Greeks and almost never as Hellenes, which would be too major a concession to their contribution to the common European heritage). Bearing in mind the image of this sober, pragmatic and European nation, the nineteenth-century ethnographers prepared the first major collections of folklore and ethnographic materials, the first codifications of the traditional culture, and the first selections of what was considered appropriate to become part of the official cultural heritage and what was not. Phenomena associated with mysticism, ecstatic rituals, thaumaturgical icons, village visionaries, often were dismissed as influences from our Asian, thus mystical and superstitious neighbours. This understanding of the essence of Bulgarian Volk helped the first engineers of the nation overcome the dilemma between tradition and modernization, and modernization was interpreted as a return to the true heritage of the nation. After the constitution of a modern Bulgarian state in 1878, the ethnographers and folklorists kept combining their efforts for collecting and preserving the tradition with a mission to educate, modernize and enlighten their countrymen, and expected the field ethnographers to combine similar tasks. The programmatic paper 'The Importance and the Tasks of Our Ethnography', published by Ivan Šišmanov in 1889, was addressed to educated people living in the countryside and urged them to collect folk materials in the field in which they exercised their professional activities, while explaining the fallacies of these superstitions and proclaiming the scientific truth. Traditional culture was considered a treasure, and as any treasure, it was to be withdrawn from circulation and properly stored in the newly established, modern museums and academic volumes. On the influence of Šišmanov's work, see Valchinova 2004, and Milena Benovska's chapter in this volume.

collectivized peasants, the youth volunteer groups, and the communist underground movement of the early 1940s (Markova 1980; Popov 2003: 8). All of them were supposed to represent important trends and promises for the future, or – in the case of the communist resistance groups – a model of the everyday life of the coming communist society.

In 1954 Romanska and Georgieva published a monograph on the bit and folklore of the communist partisans before 1944 (Romanska and Georgieva 1954; see also Koev 1962). Hristo Vakarelski published a study on the bit and everyday life in three villages with cooperative farms in 1956 (on Vakarelski, see Benovska's chapter in this volume). The process of forceful collectivization was most intensive in 1956–8, and his study could be considered as an exercise in applied anthropology. Ivanička Georgieva published articles on the bit of the working class in Sliven and a pioneering research on the history of Pernik miners (Georgieva 1968, 1977) Rajna Peševa published several books in the 1950s and 1960, paying specific attention to the role of the women in the family bit (on Peševa's work, see Minkov 2004: 45–9). The culmination of all these efforts was the expedition in the Burgas region in 1966–7, whose results were published in a volume edited by the director of the Institute of Ethnography, Prof. Hristo Gandev (Gandev 1970). The team carefully investigated the housing conditions, canalization, running water, furniture, the hygienic practices of the villagers, their clothing and eating habits (if everybody used his own cutlery and dish, or if the whole family dipped their bread in the same dish), bodily hygiene and sleeping habits (if they used clean sheets), literacy, entertainment, and how the villagers spent their free time. Then it issued detailed recommendations for improving the bit of the villagers, starting with the village street system, and finishing with the clothing style of different generations. The common argument in their analysis and recommendations was the hygiene and health conditions. It was a clear modernization approach, and the standard quotations of Marx, Engels, Lenin, and Bulgarian communist leaders Dimitrov and Živkov, are practically missing from the report. The concept of bit acts here as a practical tool for state intervention in everyday life.

The concept of bit also provided a framework for classification of social groups beyond, or even against, the class analysis of society, which was the only officially acceptable approach in that period. Several authors discussed the social groups whose 'bit' most deserved intervention from the state. A favourite object in the 1950s was the youth, suspected of being tempted by 'things' coming from the West, by the beautiful objects for consumption. The primary target in the 1950s and 1960s was the village. Rajna Peševa explained that the village is the last to absorb new ideas. The elderly people, with their biological relationship with the capitalist and/or

pre-modern past, also required special attention. Women were considered more open to the influence of the old bit than the men. 'Usually it is the wife who plays a dominant role in the construction of the family bit. Quite often she became a tool for introducing old customs in the family', argued Rajna Peševa in 1963. The wife was usually born in a village (the text was written amidst the great exodus from the village in Bulgaria), she often did not work and therefore remained outside the main channels for socialization of the socialist citizen. Staying at home, she focused all her attention on domesticity and the bit, and neglected her personal edification. Peševa issued a strong warning against such wives.

> The spiritual conflicts between spouses, their mutual alienation, which bring sexual infidelity, family conflicts and the destruction of the family, are quite often preceded by the fact that one of the spouses, and usually this is the wife, is more backward. She sticks to her retrograde ideas, she is open to a naïve religiosity, superstitions, and false notions. She is unable to understand at which point she became retrograde, and she starts bringing her own retrograde ways in the bit of the family. For her husband, this is a proof of the backwardness and ignorance of his partner. From here only one small step towards the dismantling of the family (Peševa 1963a: 57).

Youth, peasants, ethnic minorities, elderly people, women. A list of groups whose everyday life required special modernization efforts from the state was emerging. And workers were added to the list, too. 'The workers who are doing non qualified work, the hands, are supposed to spend more time in production units and lack time, energy and conditions to improve their bit, to spend their time in a more civilized way, and to work on their own education. It is precisely amongst such people that thrive vices such as alcoholism, family violence, card-playing, superstitions, and religiosity' (Conkov 1965: 55). Apart from the religiosity, the image of the drunken, card-playing, semi-literate worker, who beats his wife, is not particularly original, and would seem a commonplace in a nineteenth-century West European text discussing the conditions of the lower classes and the roads to their enlightenment. What makes the text unusual is that it was published in an official edition, addressed at low-level staff of the socialist institutions, and its author was a professional propagandist, professor of the Academy of Social Sciences and Management, training top cadres of the Communist Party – the party of the supposedly ruling working class. The concept of bit allowed for a classification of social groups independently of the class, and justified state intervention in the everyday life of people like the workers, who were officially considered the model and the rulers of the new society.

This classification helped in the division of society into hierarchical categories, following an evolutionist ladder. The bottom of the ladder was occupied by groups who were considered the most backward, and lagging behind in the effort to modernize society as a whole, and therefore requiring direct intervention of the socialist state. However, this implicit classification contradicted the classification by class, equally hierarchical and evolutionist construction and nominally the only one acceptable in the framework of the socialist state. In the official classification by class, the top of the ladder was reserved for the working class, the ruling class whose interests were represented by the Communist Party and whose ethos was to be imitated by the less-developed classes; the honorary second place was held by the peasants, whose interests were represented by the Agrarian Party, an official ally of the BKP, and only then followed the 'civil servants', a residual category including bureaucrats, teachers, doctors, intelligentsia, etc. The implicit hierarchies based on bit offered a quite different picture and the working class and its peasant allies were usually classed after civil servants. Moreover, the classifications based on bit, being implicit, were more flexible than those based on class and shifted according to the current priorities of the socialist state.[7] For example, during the peak of the efforts to create a mono-ethnic society, the Roma were sometimes referred to in official documents as 'Bulgarian citizens with poor culture of bit'. Or when the BKP became increasingly concerned with the attraction of the alluring images of Western consumerist culture on the fragile minds of certain socialist citizens, Geno Conkov offered a new classification of the 'survivals' in the bit, starting with the relatively innocent legacies from the pre-feudal and feudal period, and ending with the especially dangerous 'survivals' from the capitalist period (Conkov 1988: 122–34). The ingenuity of this taxonomy was that even the latest Western consumerist fashions were automatically classed as 'survivals' from a period preceding socialism.

Bypassing the Concept of Ideology

The concept of bit turned out to be a useful tool for evading the notion of 'ideology', another cornerstone in the framework of socialist policy. The official definitions of ideology from that period always stressed the relationship between this 'system of views and ideas' and a particular class. 'In a class society the ideology always has a class character', announced the

[7] Even when participating in the construction of these hierarchical taxonomies, the ethnologists were most reluctant to oppose one part of the popular culture to another. When making distinctions was the order of the day, the usual approach was to classify the different groups according to an evolutionary ladder, instead of discussing them as autonomous entities.

official *Encyclopedic Dictionary of Philosophy* in 1983 (Kelle 1983: 199–200). This concept had obvious shortcomings when the socialist state had to intervene in the everyday life of supposedly impeccable classes. This became especially apparent in the state policy towards religion. Until the late 1950s, the official documents clearly classified religion as specific 'reactionary ideology', opposed to Marxism. In 1946 the first Bulgarian communist leader, Georgi Dimitrov, defined religion as ideology (Dimitrov 1954), and as late as 1957, the Politburo of Central Committee of the Bulgarian Communist Party issued a series of recommendations for 'the ideological fight against reactionary religious ideas' and for the prevention of the spread of 'religious ideology' (Rešenie 1986: 8). This classification was perfectly in the tradition of Soviet Marxism, and the pre-war Bulgarian Marxist tradition. However, since the late 1940s, the Bulgarian authorities have become increasingly concerned with the fact that the profile of the typical religious believer does not fit with the expected profile of a class enemy. Workers and peasants seemed more open to the influences of religion than the civil servants, or even the few remnants of the bourgeoisie.

The Fifth Congress of the Bulgarian Communist Party in 1948 noted that religiosity was especially strong in the villages, and the same statement was repeated during the whole socialist period by various authors. 'The backwardness in general and especially in the field of religion is especially strong in the villages', argued Conkov (1965a: 55; cf. Karačiviev 1976: 12). The same was repeated about the regions most distanced from the big cities, especially the mountain regions. Elderly people were considered especially open to religious influences, because elderly people go together with elderly ideas explained Rajna Peševa (1963: 52). The ethnic minorities tended to be more religious than the majority (Conkov 1965a: 55; see also Savov 1977). And women were considered more open to religious influences than men (Peševa 1959). When there was an accumulation of several such characteristics, for example an elderly woman in a mountain village, the case was considered practically lost from the very beginning, and some authors even recommended clemency. 'An elderly woman often starts seeking compensation for the lost social relations, and she finds it in religion', argues Stoimenova (1982: 42–3). Characteristically, the workers joined that group and one author complains about their tendency to take sick leave on important religious holidays (Stojčev 1957: 101).

An enormous sociological survey was conducted in 1962 on religiosity in Bulgaria, under the direction of Prof. Živko Ošavkov: 42,664 people were interviewed (Ošavkov 1965: 311). It turned out that the elderly were more open to religious influences than the young, there were almost twice as many religious women as men, and more than 81 per cent of the illiterate

were identified as religious against less than 8 per cent of people with university degrees, while there were twice as many believers amongst the ethnic Turks as amongst the ethnic Bulgarians, and the peasants and workers were more religious than the civil servants (Ošavkov 1965; see also Stojčev 1968; Krãstev 1974b, commenting on the same survey). The working class was again put under scrutiny. Červenska explained in 1972 that the workers in cooperative farms and the services were real 'bastions of religion' (Červenska 1972: 76), while Krãstju Krãstev quoted a survey indicating a high degree of religiosity amongst the workers from the transport and construction industry (1979: 11). The key concepts, used by the socialist state for analysing society and designing its policy, the concepts of class and ideology, were obviously irrelevant in this situation. Large social groups with impeccable class origins were open to alternative ideas to the official communist ideology. It was a cognitive disaster.

One option was to bypass the concept of class by investigating the personal qualities of the believers, not their group/class affiliations. Religiosity is a natural and inevitable phenomenon amongst the weak, the ignorant and overburdened with work, explains Todor Stojčev (1957: 89–93). Other specialists, quoting Soviet authors, offered more sophisticated typologies. Summarizing contemporary research, Mizov identified religious people as introverts, as opposed to extroverts, and melancholic and phlegmatic, as opposed to choleric and sanguine (Mizov 1977: 134–9). However, this approach, never became the basis for any policy of the Bulgarian socialist state, which usually preferred taxonomies based on groups, not individuals.

Another interesting endeavour was to reclassify religiosity as half-ideology, a specific type of ideology that might coexist with other dominant ideologies within the same group (e.g. Stojcev 1974; see the criticisms of Mizov 1977: 55–62). Again, it did not work well. The conceptual problem remained – how to think of 'a system of opinions and ideas' that was both opposed to the official one and typical for the impeccable classes of peasants and workers, and how to name the problem, in a totalitarian state which interpreted everything in terms of class and ideology. A solution was offered by Prof. Živko Ošavkov, who used the concept of bit creatively. In his analysis, published also in Moscow, he argued that a strict distinction should be drawn between the religious survivals in the domain of social consciousness, and those in the domain of bit. He claimed to be the first socialist author to draw such a distinction (Ošavkov 1965: 325). The core of his argument was that the religious survivals in the domain of bit do not entail personal, conscious involvement. They should be treated as procedures, implemented mechanically, without personal implication. The religiosity in bit is 'not deep convictions, but habits', he wrote, and habits in a Pavlovian

sense (Ošavkov 1965: 328). This theory was based on a creative oversimplification of the concept of bit. A more sophisticated ideologist from Moscow might have been reluctant to introduce a clear distinction between religion and ideology. A native Russian speaker, used to the opposition between 'byt' and 'Bytie', might have hesitated before putting religion in the domain of bit. On the other hand, in Bulgarian social sciences of the 1950s and 1960s, the term had lost much of its original pre-Soviet Russian connotations, and the everyday words associated with it often derived rather from the Turkish 'bit', flea, and not from the Book of Genesis.

This creative use of the concept of bit had obvious advantages, and quickly became the basis for the state policy towards religion. In 1963 and 1965 Prof. Ošavkov recommended focusing the anti-religious campaign against the infrastructure of religion, against the rituals, interpreted as habits – mechanical gestures and procedures – and identified by him as belonging to the domain of bit. The Politburo of the BKP adopted a resolution in that sense in 1965, and in December 1966 the Central Committee of the Communist Party voted for a resolution 'for further improvement of the atheistic work amongst the working class', based on the 1962 survey (Barãmov 1972: 14). It was safe already to use the term working class, because the bad practices of religiosity were identified as belonging to the domain of bit, the everyday practices, gestures and habits, and not to the domain of ideas. The conceptual impasse was overcome and the follow-up was logical. Religiosity was added to the list of backward practices in the domain of bit and in 1967 Prof. Rajna Peševa wrote that the backwardness in the domain of bit had several dimensions – in cooking practices it was manifested by preparing oily and salty dishes; in housing by constructing damp rooms and houses; in personal hygiene by forgetting the everyday brushing of teeth, by not washing hands after visiting the toilet, by not taking regular bath or changing the bedclothes. The backwardness of bit, she went on, is displayed by a taste for vivid colours and plastic things. In family relations, the backwardness in bit is revealed by men beating their wives, by wives gossiping behind their men's backs, and by religiosity, 'one of the survivals from the past, most revealing the backwardness of people, a heritage from the class societies' (Peševa 1967: 17). Several leading ethnographers took part in designing new rituals, to replace the old religious ones (Petrov 1998, 2000), and in fighting the remnants of religiosity in bit, as part of the effort to modernize everyday life and Bulgarian society as a whole.[8]

[8] For a stimulating discussion of the real alternatives of these transformative efforts, see Amir Weiner 1999. When the socialist state lost confidence in its transformative powers, the alternative was often physical elimination.

Ethnos and the Research on the Origins of the Nation

Meanwhile, important changes occurred in the institutional structure of Bulgarian ethnology. One of the most interesting processes of the period was the gradual re-affiliation of ethnography and folklore from the domain of languages to the domain of history, as it was in the USSR (see also Milena Benovska's chapter in this volume). After 1944, the newly established Institute of Ethnography was classified as belonging to the language section of the Bulgarian Academy of Sciences. Its first director came from the Department of Slavic Languages, and the bulk of its staff – from the Institute for a Dictionary of the Bulgarian Language. A major shift towards history began by the end of the 1950s. A new director was appointed, a professional historian; the institute was transferred to the history section of the Academy of Sciences, and the university education in ethnography was gradually transferred to the Faculty of History.

This process reflected similar transformations in the USSR, where ethnography was also heavily dependent on history (for similar developments in Poland, see Posern-Zielinski 2005: 115). However, in Bulgaria quite different issues were at stake in comparison to the USSR. Like their Soviet colleagues, Bulgarian ethnographers also began to investigate the ethnic origins of the nation, in close cooperation with (or rather under the direction of) historians, but this became part of a more general shift towards nationalism. Since 1958, the Bulgarian Communist Party has proclaimed an end to the class war, and Bulgarian society to be composed of mutually friendly classes. This combined with a re-evaluation of Bulgarian history, and the academic editions began to discuss the differences in the ethnic origin of Bulgarians and the rest of the Slavic nations, including Russians. While in 1954 the first edition of the official *History of Bulgaria* stressed the Slavic origins of the nation, thus symbolically constituting it as belonging by its very essence to the same family of nations as the Russians, the second edition in 1961 mentioned the role of the Proto-Bulgarians, a nomadic tribe of probably Turkic origin which established the early Bulgarian state in 681, as an important part of the ethno-genesis of the nation (cf. Bourmov 1954: 66 and 1961: 58–63). Soon it became quite normal to discuss the relative importance of Proto-Bulgarians and Slavs in Bulgarian genealogy (Gjuzelev and Dražev 1966: 4). Since the mid 1960s a third component has been added to Bulgarian genealogy, the Thracians, a group of tribes inhabiting the eastern parts of the Balkans in antiquity. A department of Thracian studies was established at the University of Sofia, and a homonymous institute was inaugurated in the Bulgarian Academy of Sciences. In 1980 the academic 'Ethnography of Bulgaria' fixed the roles of each of these progenitors in national heritage. The Slavs were democratic egalitarian peasants (Primov

1980a: 173), the Proto-Bulgarians were warriors (*ibid*.: 178–9) and creators of the new state (Primov 1980b: 182) and the Thracian heritage has been found mainly 'in religion, beliefs and customs' (Primov 1980b: 185), and all of them amalgamated into a new Bulgarian nation. The culmination of this process of stressing the unique and glorious historical background of the Bulgarian nation state was the celebration of its 1300-year anniversary in 1981.[9] The reshaping of the institutional structure of Bulgarian ethnography after a Soviet pattern – establishing a closer institutional link between ethnography and history, and a shift of research focus towards the genesis of the nation and its historical development – served different ends in the two countries and the messages they produced were also different.

The shift of research interest towards national genealogy was marked by the concept of ethnos. In the 1970s, Bulgarian ethnography largely adopted Julian Bromlej's definition of ethnos, and the *Ethnography of Bulgaria*, published in 1980–5, is structured around this concept. This official synthesis of the achievements of Bulgarian ethnography, and the use of the concept of ethnos, is analysed extensively by Asen Balikci in this volume. Here I would like to stress only the different uses of the concept in Bulgaria and the USSR. In Soviet ethnology, ethnos was used as an intellectual tool for classifying the processes in an evolutionary perspective (cf. Hirsh 2005), in Bulgaria the concept of ethnos was applied mostly to analyse the processes of amalgamation between Proto-Bulgarians, Slavs, and Thracians, which presumably ended by the end of the tenth century (Angelov 1981). The tribes and nations, migrating after the tenth century, are not included in the national ancestry, and the story of Bulgarian ethnos is presented as a continuous struggle for self-preservation against a variety of invaders. The culture of Bulgarian ethnos was also treated as established once and for all by this process of amalgamation, and serious attention was paid to demonstrate its resistance against the impact of other cultures, especially during the Ottoman rule. Further, in Soviet ethnography the concept of ethnos was used to classify the complex processes in a multinational empire. In Bulgaria, it marked the efforts to construct a mono-ethnic nation. The publication of the last volume of *Ethnography of Bulgaria* in 1985 coincided with the process of forceful renaming of the Bulgarian ethnic Turks (launched in December 1984), who were required to adopt Bulgarian names (on the reactions of contemporary ethnographers and folkorists to the process, see Vassil

[9] An interesting development in the Slavic part of Bulgarian genealogy was the pronounced effort to symbolically invert and 'englobe' (cf. Herzfeld 2005: 140) the dominant Soviet influence, by claiming that the Bulgarians were the first ones to provide a model to the Russians, by introducing them to the Cyrillic alphabet and orthodox Christianity.

Garnizov's chapter in this volume). The process culminated with the exodus of some 300,000 ethnic Turks from Bulgaria.

The closer institutional link between history and ethnography, the intensified interest in the origins of the nation and the ethnic processes, the domination of the concept of ethnos – all these trends have their formal parallels in Soviet ethnography, but were adapted to local uses and served the nationalistic policy of BKP. Meanwhile, the concept of bit was not withdrawn from circulation. While struggling to construct a homogenous, mono-ethnic Bulgarian society, the socialist state has not resigned from its efforts to modernize the everyday lives of the presumably backward groups. The ethnologists, working on bit, collaborated closely with the National Front (henceforth OF).[10] They were mostly concerned with practical solutions related to the modernization of everyday life, including campaigns against the remnants of religiosity amongst backward social groups (Hadžinikolov 1980). To their colleagues from other social sciences, involved in the campaigns of the OF, ethnography seemed primarily a science of bit, not of ethnos, and envisaged its further evolution mostly in that direction. We need, wrote Nikolaj Mizov, 'a specific general science on bit, which we could call *bitology*. A second science could and should investigate the bit in the precisely delimited boundaries of a nation. This specific, concrete science of bit of a nation is represented by *ethnography*' (Mizov 1976: 41). The comparative science dealing with the everyday life and culture of various societies is defined here as 'bitology'.

[10] The OF was created in February 1948 by an amalgamation of several political parties, allied to the BKP after 1944. Later it became an umbrella institution for a variety of organizations, so by the 1960s practically every adult citizen who was not enrolled in the Communist or Agrarian parties, or the Communist Youth Organization, was considered a member of the OF by default. It specialized in softer forms of activism, like public talks, lectures, or petitions and addressed mostly social groups, which remained outside of the reach of the basic structures of the Communist Party, such as elderly people, youth, housewives, etc. Being based on production units, the Communist Party structures experienced specific difficulties in getting in touch with unemployed people. The OF was expected to bridge this gap: its own structures were established in *territorial units*, where they were able to reach the unemployed, people above or under working age, or adult citizens *outside of work hours*. Historically, the evolution of the OF was based on this early division of labour and it became specialized in managing the territorial units and the free time. Its development followed the transformation of official policy regarding the bit and everyday life. A variety of functions were added to OF, like management of the public spaces, streets, squares, and green areas (1957), voluntary work in several sectors of the economy (1963), improvement of general healthcare, education, sanitation of urban and rural areas (1967), collection of paper, metal, and plastic scrap, campaigns against alcohol abuse, preservation of the national traditions, promotion of family values, mediation in conflicts between neighbours, etc. All of these functions were more or less related to everyday life, conceptualized in that period as bit. See also Stojnev 1985; Brunnbauer 2007: 395–426.

Both bit and ethnos perfectly fitted the policies of the socialist state. 'Bit' was a useful tool for drawing internal distinctions and hierarchical taxonomies for a policy of modernization, and for planning state intervention, while 'ethnos' implied homogeneity, corresponded to the increasing nationalism of the Bulgarian Communist Party, and justified the efforts for creating a mono-ethnic and classless society. Scholars profited from different institutional developments, those working on bit were largely incorporated in the efforts of the OF, while their colleagues who opted for ethnos benefited from the reclassification of ethnography as sub-division of history.

Ways of Living and Socialist Individual

A third concept, a new group of scholars, and a new institution joined bit and ethnos in the early 1970s. By that period, the socialist state had become increasingly interested in the personal motivation of the citizens. The tenth party congress in 1971 envisaged a more generous set of personal stimuli for the workers, in order to increase productivity, while the December Plenum of the Central Committee of the BKP in 1972 officially proclaimed welfare as the 'supreme objective of socialist production efforts, and of the development of the new social order' (Živkov 1972: 5). The party wanted citizens to be hard working, satisfied, and motivated. These efforts required new intellectual tools, and the concept of 'way of living', or especially 'socialist way of living', was borrowed again from the USSR in the 1970s. While 'bit' focused on material infrastructure and justified direct state intervention, the concept of a 'way of living', implied a preoccupation with individual motivation and stimuli.[11] The definitions of a 'way of life' usually included notions like 'system of moral values', 'personal motivation', and 'everyday activities' of socialist citizens (see especially Angelov 1980, 1985). In parallel, the minister of culture, Ljudmila Živkova, daughter of the socialist dictator Todor Živkov, initiated large scale programmes for constructing a 'harmonically developed personality', and for promoting individual self-development and creativity.

It was in that context that the Institute of Folklore was created. Its young staff had background mostly in languages, philology and the arts, and opposed both the historicist interpretation of their colleagues from the Institute of Ethnography, and their preoccupations with the material infrastructure of everyday life, typical for the proponents of bit. Instead, they focused their attention on the folk artists' individual acts of creation, and folklore as

[11] For a different development in the former DDR, where 'byt' was amalgamated with 'way of living' into the concept of *Lebensweise*, see Noack and Krause 2005: 39–41.

a domain of authentic inspiration and creativity. The process is analysed in detail by Vassil Garnizov in this volume.

In parallel with its efforts to motivate socialist citizens, the Bulgarian government persistently implemented a policy of a homogenous nation, and the persecution of ethnic Turks, Pomaks and Muslim Roma are among the darkest pages of the whole socialist period, probably the darkest one. The efforts of the OF aimed at modernizing the everyday lives of the backward social groups also continued. The debates within Bulgarian anthropology largely reflected these trends. The '*ethnos*' hardliners, promoting an image of a homogenous nation, clashed with the '*popular creativity*' clan from the Institute of Folklore, who promoted the image of popular artists and artisans as genuine creators, true individuals, fully-fledged personalities. Emotions ran high, and the repercussions of the loves and hatreds of that time are still felt strongly today. The debate opposed supporters of the homogenous nation, rooted in history, to advocates of the creative individual, the popular artist, presumably able to express a unique personality and at the same time to sing in perfect harmony with millions of other equally creative and anonymous individuals. A concept justifying ethnic cleansing competed with a concept justifying the idea of a harmoniously developed personality, both of them perfectly fitting within the policies of the totalitarian state. The debating parties invoked one Soviet authority against another and against their colleagues, who kept working on bit.

Concluding Remarks

The policies of the socialist state towards important issues like modernization, management of everyday life, ethnic minorities and personal motivation were far from coherent. This provided room for choices and genuine debates concerning the important trends in Bulgarian society, and alternative approaches were anchored in competing institutions within the framework of the Bulgarian Academy of Sciences, universities and museums. All these processes occurred behind the seemingly monolithic façade of the totalitarian state and ideology, which never acknowledged their own transformations, and tried to present themselves as timeless, fixed and eternal. The very image of immobility that the official ideology tried to implant prevented any open discussions of the key analytical concepts, but this left ample room for local modifications of intellectual tools coming from the socialist metropolis. Bit, ethnos and way of living were adapted to the needs of Bulgarian socialist state, sometimes moving far away their Soviet prototypes. Bulgarian ethnologists enjoyed a certain degree of freedom, being able sometimes to manoeuvre between mutually contradictory aspects of official state policies and to alter, without openly challenging them, the seemingly fixed concepts

of official ideology. Much like the peasants, described by Gerald Creed (Creed 1998), they were able to choose which among the contradictory state policies to follow, and tacitly to discard the rest, which (intellectual) tools to use, and which ones to oppose. However, the situation within Bulgarian Academia seems to require a more sober interpretation than that offered by Creed. The choices made by Bulgarian ethnologists referred more to tactics rather than strategies in the sense of Michel de Certeau (2002 [1990]: XLVI). The local actors were not able to introduce dramatic changes in the field, to invert the hierarchical relations of Soviet ethnography, or to modify the priorities of the socialist state. All they were able to do was to use the opportunities provided within frameworks defined by socialist state and ideology, and on some rare occasions to navigate between the clashing teams and to create relatively independent intellectual spaces outside the raging debates.

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Chapter 8
The Debate between Folklorists and Ethnographers in Bulgaria (1975-1985)

Vasil Garnizov

Aim

The direct drive for writing this article came from Ilia Iliev, and indirectly, from Chris Hann, but my real motivation stems from a paradox for which I do not have even a preliminary explanation: there were scholarly methodological debates in Bulgaria at a time when the ideological context did not stimulate them, whilst there are none today when the environment encourages such debates. I do not take a neutral position in these debates, even though I have not participated directly in them, but my objective here is not to defend the validity of any position. I believe the fact that the debate existed, regardless of the actual content of the arguments, led to a constant problematization of existing scholarly and ideological 'truths', and created conditions for a certain freedom of choice as to what we could research and what methods we should employ. Now the freedom is available but the methodological vigilance is absent – we know what our colleagues argue and what they know, but we do not know *how* they know it. When it comes to fundamental, conceptual, methodological or technical debates, the public preference is to banalize it rather than to problematize.[1] Therefore, the aim of this article is to present very schematically the debate that took place between ethnographers and folklorists during the last decades of socialism. In

[1] For example, Račko Popov, an esteemed colleague of mine and director of the Institute of Ethnography and Museum, recently stated that in 1959, Hristo Gandev 'oriented ethnography to the realm of the historical disciplines', which received the following comment: 'This historical orientation of ethnology and the prevailing study of it at the historical departments of Bulgarian universities seriously worried a number of representatives of the philological stream, mainly in folklore studies, almost up until the end of the last century' (Popov 2003: 10). At the same time, in ethnographical circles there were other views on the benefit of super-historicity of the discipline. For instance, Radost Ivanova, chief editor of *Ethnologia Balkanica*, stated that, 'Long held in the realm of history, ethnology today is split between the past and the present' (Ivanova 2002: 14–15).

addition, this article will try to understand why this debate does not continue today, and why the role of folklore studies and ethnography during the socialist period is not discussed at all.[2] I will start with the latter.

About a Critical Reading that did not Happen

The changes in Bulgaria at the end of the last century were accompanied by unsuccessful attempts to revise the role of the social sciences as ideological tools of the socialist rule. Since 1990, Bulgarian and foreign scholars have raised – from different perspectives – the question about the ideological and political use of folklore studies and ethnography. Situated internally, the topic usually fell into the context of a fight for power in academic circles; the issue turned into a personal one – as for instance, was the case with Prof. Valentin Angelov against the director of the Institute of Folklore, Todor I. Živkov.[3] When the issue was raised from outside, as Klaus Roth[4] did, it

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[2] I cannot be entirely neutral when presenting this argument as I am a student of Tudor I. Živkov: he was my MA thesis and dissertation adviser. I have worked at the Institute of Folklore at BAS: Živkov guided me, gave me many books that were not available in those years, patronized and encouraged me. We had excellent relations until 1985–6 when they cooled off and became strictly official, at times even hostile, because I refused to deliver a paper or a speech at a conference on the process of the forceful renaming of the ethnic Turks, given that I had certain fieldwork experience in Turkish and Pomak villages in the eastern Rhodopes from 1976 and 1985. (Subsequently, I continued my fieldwork in 1991, 1993 and 1996). This had an impact on my academic career – I defended my Ph.D. dissertation only in 1993.

[3] V. Angelov defines the debates about the border between ethnography and folklore studies as 'unserious pursuit, stemming from situational and methodologically false premises'. He criticizes the theory of folklore as a type of culture, as well as Živkov's view on the arts of folklore (Angelov 1999: 13, 21–3, 210, 211). For a short period – until the adoption of the law on decommunization of science – Angelov was very active: he published writings about the ideological abuse of science; he wrote to the chairs of the Bulgarian Academy of Science and others. It should be noted that Angelov was a member of the editorial board of the journal *Bulgarian Folklore* but he failed to transfer the debate onto its pages since Živkov was the chief editor of the journal at the time.

[4] 'Long, even far too long, folklore studies ... had to be subjected to the socialist and ethnocentric ideologies; it had to put its studies to the service of state goals and intentions' (Roth 1992: 38–9); it pointed out 'fictitious issues such as studies on the socialist way of life' *(ibid.:* 47); he called for 'not allowing for ethnography and folklore studies to play the role of auxiliaries in the cultural management of the people' (*ibid.*: 47); he criticized the to him unacceptable 'division of folklore studies into ethnography and folklore' (*ibid.*: 48); 'the study of the people's culture should be analytical and theoretical, reflecting its own foundations and interests' (*ibid.*, p. 47). It should be noted that contrary to Valentin Angelov's case, Roth's article was published with Živkov's approval in his capacity of a chief editor of *Bulgarian Folklore* and director of the Institute of Folklore – the external criticism by Roth was not personal. Nevertheless, Živkov did not pass by Roth's publication in silence: by using word play, he compared his imperatives to the ones that Moscow tried to impose on Bulgarian

sounded more general and set itself more ambitious goals: to initiate a debate, to encourage self-reflection and to outline new venues for the development of scholarly studies.

These attempts did not receive a widespread response. Angelov's efforts were banalized and Roth's appeals were greeted with silence. Ethnographers and folklorists began to rename themselves as ethnologists and social or cultural anthropologists.[5] Meanwhile, the National Assembly passed a law at the end of 1992 that banned former party and state activists from taking leading posts in the academic and educational institutions. Thus the disciplines were partially delegitimized and the directors of institutes, the deans and chairs of faculties and sections made redundant. Expectations for subsequent self-reflection on the scientific toolkits, methods, concepts and theories in ethnography and folklore studies emerged. However, in reality this did not happen – instead of a critical reading of the recent past, ethnographers and folklorists preferred to look for new legitimacy and a new future in the motley and uncritical acceptance of western scholarly traditions.

The reasons for what happened are not easy to present briefly, nor to be understood impartially, due to the short lapse of time since their occurrence. The effort, however, is worthwhile, because the failure of the project to encourage self-reflection is informative both of the present and the past of the sciences of the people.

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folklore studies: 'paradigm M.' in its two meanings – 'Moscow' and 'Munich' – Živkov 1999: 105–22). In passing, it should also be noted that Roth's criticism did not trespass beyond certain boundaries. For instance, he did not pose the question about the forced renaming of the Bulgarian Muslim population (Turks, Pomaks, Roma) and the abuse with science regarding the argumentation of this state policy anyway. What is more, a certain dose of ahistoricism can be seen in his efforts, which, to a certain degree, weakens his moral appeal: the main analogy with the statute of the sciences under socialism was the one with the time of Nazi Germany, and the main positive example – Bausinger. However, Bausinger began to publish his critiques of *Volkskunde* in 1961 and expanded them in 1971 (i.e. more than twenty-five years after the first phase of the denazification of Germany) when a large part of the old elite had withdrawn. The year 1968 had passed and a new generation had stepped onto the public scene. It should be noted too that recently Roth has stated the difficulty of imposing the term 'everyday culture' from abroad (Roth 2001). In addition, he softened the imperative style of prescriptions as to how the disciplines should develop and he tried to combine the internal and the external points of view, the etic and the emic (Roth 2003).

[5] These processes were described by Magdalena Elčinova, but again they were directed to foreign readers and a foreign environment (Elčinova 2002a, 2002b, Forthcoming). I, myself, wrote – for an external readership too – that ethnology took part in the creation of the ethnos, and did not merely study it and I pondered briefly over ethnography and folklore during the socialist period (Garnizov 1992a). I have done that in front of a Bulgarian scholarly audience right after the beginning of the changes, but the collected volume of the papers was never published due to a lack of financing (Garnizov 1992b). Nevertheless, it received a response (Raycheva 1993), but there were no further consequences.

Firstly, we need to consider the new academic status quo and the controversial effects of the law on the decommunization of science and higher education. Internal personal criticism lost its role after the removal of the former administrators of science, and the criticisms coming from abroad only improved the reputation of the concerned scholars and turned them more into opponents rather than supporters of the project for the reinterpretation of the past. It seems that the newly elected leaders of the Bulgarian Academy of Science (BAS) and the universities did not have a real motive to rethink the past. By implementing the law they won by default the battle for academic positions in which they did not take part. Despite its brief effect, the law helped an entire new generation to take on the power in the academic field, which exhausted the energy and resources for a debate between the generations. After that their main concern was maintaining the new status quo, which took place predominantly at BAS. The old professors were kept in the scientific councils in order to make them correspond to the formal requirements for scientific legitimacy. The organizational rulebook of BAS was modified so that the academicians became life-long members of the General Assembly, and the use of mandates in the selection of the directors of the institutes, the chairs of the scientific councils and the heads of sections (Ustav 1994), was abolished.

Secondly, the decommunization of sciences was brief and inconsistent. Effectively, the law was in force until April 1994 and was officially rescinded in March 1995, when the successors of the former Bulgarian Communist Party (henceforth BKP) – renamed Bulgarian Socialist Party – took power. At the same time, the decommunization of science and higher education in Bulgaria was not accompanied by the decommunization of other institutions – the state and local administration, the courts, prosecution, the secret services, economic enterprises, the banks. The old elites kept their influence in the public and economic spheres. They kept it in scientific circles – formally, as the law did not forbid them to be chief editors of scholarly publications, as well as informally. Scholars who were part of the pre-1944 elite and who were connected to these elites with biographical, moral and corporate ties were elected for leading posts.

Thirdly, despite the fact that during this short period 2,830[6] people were removed from leading posts and membership in faculty and scientific councils, the newly elected were not motivated to adopt a critical distance vis-à-vis the recent past. There was an atmosphere of institutional instability; faculties and institutes were shut down; the budgetary maintenance of the organizations was drastically reduced, and the lay-offs of scholars were

[6] http://www.geocities.com/decommunization/Articles/Hristov4.htm.

widespread. There were rumours about shutdowns and merging of institutions with the argument that they were created for political purposes and served political and authority interests.[7] In this context, the attempts for a critical reading of the past of one's own institution, its objectives, methods, achievements and dependence on the political situation, use and abuse of science were perceived as a collective act of self-destruction and were stunted as early as in their embryonic form.

Fourthly, folklore studies and ethnography were not the only subjects which missed the opportunity to reconsider their Marxist heritage. In practice, the debate on Marxism did not take place in any of the social sciences – economics, history, sociology, law, etc. Marxist rhetoric became illegitimate. It disappeared from public speech and political debates, and ceased to appear in the footnotes of scholarly studies. At the same time, Marxist ideas, concepts and theories implicitly continued to influence politics, governing practices and daily life.[8]

Fifthly, in Bulgarian society itself the energy for reconsidering the past diminished and against this background the collective amnesia about the behaviour of scientists under socialism was not the most dramatic issue at all. Unresolved were the issues and lawsuits about the repressions during the communist rule. The archives of the communist secret services were not open for public access. Without solution and without convicted persons the prescribed term on the case about the so-called 'revival' process, i.e. the forced change of the identity of the Bulgarian Turks and their subsequent expulsion from the country, expired.[9]

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[7] Very often the rumours had their institutional foundations – according to my personal observations, at least three times there have been unsuccessful attempts to shut down the Institute of Folklore and to merge it with the Institute of Ethnography, whereof it originated historically. The arguments on behalf of the supporters of this idea were that folklore studies were not a separate field of science; that the Institute of Folklore was established by political persons for ideological reasons; that two institutes had one and the same subject of study and carried out the same activity; that the merger would lead to considerable budget savings for the Bulgarian Academy of Science.

[8] 'The Marxist labour theory of value continues to live in people's minds and still shapes today's Bulgarian reality' (Ganev 2005).

[9] The grounds for amnesia is a separate subject, which cannot be examined consistently here. Yet it should be noted that recently some scholars and intellectuals began to mention their behaviour during that period. For instance, Vera Mutafčieva stated that she did not make a stand against the party's authority (because Stojan's [the one who stays] mother cries, Bežan's [the one who runs away] mother does not), but in the period 1985–90 she did not attend any conferences (she 'shirked'), in order not to have to lie or keep silent and so that she would not tarnish her name (Garelov 2007). This topic was addressed briefly by some Bulgarian authors and in the foreign academic press (Elčinova 2005a, 2005b).

For these reasons, in Bulgaria today there are two extreme positions towards the recent past of ethnography and folklore studies. According to one of the positions – the position that argues for a radical breach with the past, which is defended very rarely in the country and very often abroad – everything accomplished by ethnographers and folklorists during socialism falls into the undistinguished ideological grey area; they are part and parcel of the ideological control as well as expressions of a 'harder' or 'softer' form of nationalism. According to the other position – the position of scientific continuity – by the replacement of a few rulers in science the issue about the attitude to the past was resolved, which allowed for scientific and educational institutions to be preserved as names, paradigms and approaches.

None of these major approaches to the recent past of the disciplines is very useful in view of their future. Firstly, not everything in the history of ethnography and folklore studies is ideologically grey, smooth and homogenous. Secondly, in certain periods, there could be observed a greater autonomy of expert knowledge or at least a clear division of labour between ideologists and experts. Thirdly, there were complex relationships among the disciplines themselves; debates running before 1944 continued during the socialist period, and the substance of the issues raised was the same as before 1944, although sometimes in their arguments the scholars pretended that they were competing to prove their consistency vis-à-vis Marxism.[10]

The Political Context

Such a time of dynamism and debate was the period from the beginning of the 1970s until the middle of the 1980s. The dating of this period is somewhat conditional as some processes began before and ended after the delineated time frame.[11] There was a partial opening towards the West after the Stalinist era and at the end of the 1960s, which led to the emergence of an alternative elite in the circles of the communist party itself as well as among educated people. Parallel to the opening up towards the West, official support for the values of internationalism began to weaken after 1968 and a slow but stable rehabilitation of national education at the expense of the international[12] also began. Similarly, a silent revision of the Commintern politics on the issue of Macedonia and the introduction of a consistent patriotism and 'soft' nationalism, which was met with hostility by Moscow,

[10] Chris Hann reminded us how Gellner used to read between the lines the Russian authors of that period.

[11] See the timelines in the Appendix to this volume.

[12] This took place at all levels, including in the textbooks on pre-school and elementary pedagogics.

occurred. Two confronting wings were formed in the party: one consisting of members with predominantly Russian education against the wing consisting of members with predominantly western education.[13] As their representative in the authorities, the latter – the partially new elite – recognized, and were recognized by, Ljudmila Živkova, daughter of the party and state leader Todor Živkov. The new political line was institutionally completed during the Tenth Congress of the Bulgarian Communist Party in 1971.[14] The Congress stipulated new public goals: a 'course towards a developed socialist society' was set; a key question about the 'upbringing of the new man, liberated from religious prejudices and harmful anachronisms in behaviour, customs and world-views' (*ibid*.: 212–15, 218–19) was posed; the main task of the party became the goal 'to overcome the historical discrepancy between the productive and aesthetic spheres'[15]; 'the relevance of scholarly studies for policies', 'for the solution of the socio-economic, political, ideological and cultural problems of societal development' was underlined.[16]

The Institutional Framework

When specifying this periodization, the year 1975 is important to highlight because during that year Todor I. Živkov's article entitled 'Folklore as fiction' was published. This publication not only started polemics between ethnographers and folklorists, but was to a large extent programmatic as regards the institutional establishment of folklore studies.

When the article was published, the institutional separation between the two sciences was already a fact.[17] The Institute of Folklore had been already established in 1973 as a separate unit at the Bulgarian Academy of Science, which – although it provoked some dissatisfaction – neither offered criticism nor presented a threat to the ethnographers. Without confronting their former colleagues, Prof. Dinekov and Prof. Stojkova supported this

[13] The schism between them, to a large extent, predetermined the political debates at the beginning of transition after 1989.

[14] The materials from the congress were published two years later (The 10th Congress 1973).

[15] *Ibid.*.: 214. In fact, this is how Ljudmila Živkova's star was born, namely by placing the aesthetic as the sublime form of the ideological.

[16] Quotations from the congress paper of A. Lilov, a key ideologist of the era and, after the changes, and currently a leader of the renamed Communist Party into Bulgarian Socialist Party.

[17] This issue was not a subject of special study either. One of the unofficial explanations comes from Ljubka Parpulova: according to her, folklorists were more productive than ethnographers but they had a more difficult time finding a place to publish on the pages of the old Institute.

cause publicly, while the party and state line was represented by Todor I. Živkov, who at the time was the party leader of BAS.

Therefore, at the beginning there was no substantial scholarly debate. Dinekov's understanding of folklore as a poetic verbal art of the people was in line with Herder's understanding and was not in contradiction either with the Bulgarian, Russian or Slavic traditions or with the then existing ethnographic views. The institutional differentiation of folklore studies was acceptable to ethnographers within the framework of the common reforms that followed the Tenth Congress of the ruling party and the subsequent establishment of Integrated Centres and Scientific Societies whereby ethnography was designated to the large division of historical disciplines.[18]

Following this disciplinary logic and in accordance with the literary view of popular art, folklore studies should have joined the Integrated Centre for Language and Literature. Instead, the newly created institute was incorporated into the Scientific Society for Art Studies where it coexisted with institutions that specialized in music, dance, theatre and others. Consequently, the very structure of the Institute of Folklore and the names of its sections carried artistic and aesthetic messages.[19]

The motives for this decision are not completely transparent, and additional studies are needed on the basis of documents from the period that are available in the archives of BAS as well as in the party and state archives. At the time, Petăr Dinekov was closely integrated in literary circles – he taught old literature and literature of the Bulgarian national revival and had numerous publications on key Bulgarian men of letters. Dinekov was the chair of the Union of Bulgarian Writers and it could be assumed that he would have preferred for the Institute of Folklore to be attached to the Integrated Centre for Language and Literature. However, it seems that this was perceived as a threat to the autonomy of the discipline because in a literary context folklore studies had or could have received the status of an auxiliary research tool – identical to the one ethnography had received in the framework of the historical disciplines.[20]

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[18] The reorientation of ethnography to the field of the historical disciplines had important consequences for this science. The immediate result was that ethnography opened up to observation and research of the changes in everyday life, and came close to the approach of social history. Parallel to this new advantage, ethnography was faced with the threat of being conceptualized as an auxiliary discipline – an instrument for collecting non-autonomous data which bring arguments in support of classical historical and archeological studies.

[19] See the timelines in the Appendix to this volume.

[20] This is a motive that I have personally heard about from T. I. Živkov.

The Theoretical Debate

In this sense, institutional reorganization to a large extent preceded the public theoretical debate. This debate began *de facto* with Todor Živkov's 1975 article and subsequent articles and books such as *Folklore and Contemporaneity* and *The People and the Song*, which put an end to the peaceful institutional coexistence of the sciences of the people, and problematized a series of important issues, which I will sum up very schematically without relying on the terminology of the authors of the period.[21]

From the point of view of the dominant ideology, the most important issue was the attempt to critically reexamine the research subject of folklore studies and the related (until then) dominating concepts such as 'the people's culture', 'everyday culture', 'traditional people's culture' and 'folklore' (Živkov 1987: 23–31), as well as the relationship between the people and art in general, understood as representing all that was not professional and 'high'. The prevailing class understanding of people and their culture was defined as 'too dogmatic, annoyingly banalized and unidirectional'. Within this critical perspective fell traditional Slavic folklore and ethnography, which were construed on the basis of the notion of classical folklore and admiration for inherited village cultural art. At the same time, basic distinctions and classifications such as material, spiritual and socio-normative culture became the subject of dispute. As an alternative, a systemic–structural approach was offered, presented in the disguise of Marxist phraseology as well as models in the spirit of the theory of information. The use of the very notion of 'folklore' began to expand considerably, to deviate from

[21] The correct tracing of the debate in detail and with the respective bibliographical referencing is not the aim of this article. (On the part of the folklorists, the debate was held predominantly, if not solely, by Živkov, whose views were expressed in Živkov 1970, 1975, 1977, 1981, and 1987. On behalf of the ethnographers, the debate was held predominantly, if not solely, by Hadžinikolov and Genčev – Hadžinikolov 1980, 1981, 1991; Genčev 1984, 1989b). This task requires considerably more room because over time the topics were developed, the contentions were changed, the arguments became more varied, and the number of cited sources too. For instance, the book *People and Song* was first meant to be a brief introduction to a particular study but it turned into an attempt for a more systematic and holistic construction of a theory against the class understanding of the people's masses and their art. This was followed by a development of an understanding of the national culture and its subsystems – 'a specialized and institutionalized culture' and a 'folk culture'. This was followed by an understanding of their development over time – folklore came from a culture that was most fully unfolded under conditions of unalienated labour towards the subsystem of the culture of the ethnos. This was followed by development of a theory of the ethnos and the ethnic that was in direct dispute with Julian Bromlej's theory. The last publications were entirely devoted to the ethnic and were in the context of the 'revival' process – on this issue, however, there are no longer any significant public differences between ethnographers and folklore studies.

Herder's tradition[22] and to become closer to some French (Van Gennep 1924; Saintyves 1936) and Anglo-Saxon views (Thoms 1985) and definitions. Folklore studies themselves became closer to ethnology and anthropology.[23]

Secondly, there was the issue of synchrony versus diachrony. The scientific status quo of the 1970s set as a priority the study of ethnogenesis examined in the tradition of a unilinear evolutionism, which was concordant with the Marxist view of history, as well as with Tokarev's paradigm, which was dominant in Soviet ethnography. Tokarev subjected both ethnography and folklore studies not to the study of the present but to predetermined reconstructions of the past. As an alternative, the views of other Russian scholars were offered, some of them emigrants, such as Bogatirev, Roman Jakobson, Jurij Lotman, as well as the views of French-speaking scholars such as Ferdinand de Saussure,[24] Marcel Mauss, Arnold van Gennep and Claude Lévi-Strauss.[25] Accordingly, instead of history, linguistics and soci-

[22] Herder's tradition was followed, to varying degrees, by P. Dinekov, S. Stojkova (awarded the Herder Prize), and L. Parapulova.

[23] After the changes, Živkov began to use ethnology and anthropology as synonyms of folklore studies (see, e.g., Živkov 2000) and he set up curricula on ethnology at the universities of Plovdiv and Blagoevgrad. This does not mean that Živkov followed an open and consistent pro-western orientation (although the changes of 1989 found only the Institute of Folklore – among the humanitarian and social institutes – with a contract for cooperation with a western scholarly institution, CNRS – Paris, France). He marketed his ideas by often dressing them in abundant quotations from Soviet and Marxist literature. It is also true that rather selectively – depending on the time – he borrowed ideas from western sociology, anthropology and linguistics. Nevertheless, his citations included, in addition to Saussure, Levi-Strauss, Jakobson, A. van Gennep, and F. Boas (about the conscious and unconscious in culture), M. Weber (about sociology of religion), V. Turner (about the ritual and the symbol), J. Le Goff (about medieval culture) and others – in the original or in translation from the Russian. His relations with Russian folklorists and ethnographers were complicated, at times hostile, but this is rarely noticeable in his publications.

[24] From Saussure Živkov borrowed the distinction between 'langue' and 'parole'.

[25] T. I. Živkov did a six-month specialization at the Laboratory for Social Anthropology at the College de France when its head was Claude Levi-Strauss. However, Živkov studied in the USSR as well. This detail from his biography is not sufficient *per se* to clarify the question of why it was structuralism that made use of the western tradition (which was not only characteristic of the Bulgarian scholarly situation at the time). This is a question that deserves a separate study itself but perhaps the explanation should be sought in the conservative ethos of structuralism, its social and political lack of criticism, which suited the era of the 'developed' or 'mature' socialism, a time of an established political and social hierarchy, and a weak social dynamics after a period of large migration waves and demographic changes. In that sense, the increased scholarly use of structuralistic ideas, models and authors cannot be considered a form of dissident position. At the same time, it would be misleading to think that it was safe to quote authors from this group. In most of my colleagues' memoirs of the time (such as that by Radost Ivanova) there is enough evidence for fierce ideological criticism on the part of the status quo when such references were discovered in their articles. This type of

ology were singled out as methodologically important and as disciplines close to folklore studies. In response, ethnography argued for the necessity phenomena to be studied in a wide chronological and special perspective, which, undoubtedly, was a convenient proposition in the context of the celebrations on the occasion of the 1,300th anniversary of the Bulgarian state. Subsequently, Živkov developed a more complex theory, which distinguished between genetic, structural and regional characteristics of folklore culture, and by doing so his theory integrated the historical dimension of culture (the genetic characteristics of folklore).

The third set of issues revolved around holism versus particularism. The sectoral interpretations of folklore were questioned by other disciplines such as history, ethnography, music theory, choreography and fine arts. The complex approach according to which different specialists complement one another when the syncretism of their respective cultures demands it was also questioned. As an alternative, a synthetic approach was offered, which corresponded to the systemic character of folklore as a cultural system. For their part, ethnographers responded that in this way a serious confusion was created in the field of social sciences. Some sharper remarks were not spared either, such as 'Some incompetent cultural activists demonstrate an aspiration to identify folklore with the entire people's spiritual and even material culture, and to replace ethnography with folklore studies.'[26]

Fourth, the debate highlighted tensions between collectivism and 'meritocracy' by problematizing features of popular art (previously taken for granted) such as anonymity and collectivity. As an alternative, the idea of folklore as a performing art, here and now, was suggested. In this case, the collective body needs individual performing talent, but the individual remains a creator within the framework of the aesthetics of identity (cf. Lotman 1970: 350). Hence the interest in the variety, the context of the performance, and the intimate sense which singers, dancers, story-tellers and woodcarvers put into their art. For this reason, a great number of studies and publications were subsequently devoted to the bearers of culture, the individual, the creator – to the story-teller (Georgieva 2000), the piper (Zaharieva 1987), the singers at fun fairs (Mihailova 2006), craftsmen and others, including a portrait of a mourner (Georgieva and Garnizov 1989). On

ideological criticism from the point of view of orthodox Marxism did not cease until 1989. I, myself, have a copy of a report against Živkov and 'his young followers' (including myself) containing an accusation of adherence to the contemporary bourgeois theories.

[26] See Hadžinikolov *et al.* 1981: 9–10. The ethnographers' reactions often express a feeling of robbed theories, a loss of ground, of a seized field in a theoretical and physical sense: in those years the two institutes were situated in the same building with constantly insufficient premises; the directors' offices were shared as well as the offices for the scholars and the library, although the newly established Institute of Folklore had a small library of its own.

this issue, the response of ethnography was, to a large extent, proper as this was accepted as an immanent part of art, but the art of folklore was thought of as particular: thus folklore studies *per se* were given 'a special place between ethnography and art studies' (Hadžinikolov *et al*. 1980: 82).

These major axes of the debate after the mid 1970s led in turn to a variety of other themes. First, there were issues of interpretation versus reconstruction. Oral history, family stories, legends, etc. can hardly be used as a source of reconstructing ethnic history. They must be understood in their own context as satisfying particular needs for situating individuals and collective bodies in time. In this respect, Živkov, to a large degree, relied on his own fieldwork experience during which he recorded a great number of oral historical stories which referred to the more distant (Tzar Ivan Šišman, fourteenth century) or more recent past (Zamfir Popov, the anti-fascist struggle from the mid twentieth century).[27]

Finally, the active role of researchers in provoking an interest in the people they studied and in valorizing their cultural and living experience, which was devalued by the processes of modernization and centralization, was properly taken into consideration for the first time. According to Živkov's observations as well as those of a number of scholars of folklore – for instance, Stefana Stojkova – fieldwork itself had a strong impact on the individuals and communities studied. The increased external interest in local traditions led to a change in the studied individuals' evaluation of their own knowledge and skills and hence in the 'objective' accounts of the scholars.[28]

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[27] Živkov's scholarly and party career was launched by a field study and a publication of anti-fascist song folklore, which gave him a strong party and political start (Živkov 1970). At the beginning, Hadžinikolov's career developed within the framework of the historical studies of the Bulgarian Communist Party and its leader, Georgi Dimitrov, in post-World War II Bulgaria (Hadžinikolov 1972). He also wrote manuals on atheist education (Hadžinikolov 1965), books on the history of the Communist Party of the USSR, Russian-Bulgarian and Soviet-Bulgarian relations, the impact of the Soviet revolution on Bulgaria, Georgi Dimitrov's life in Russia, and a general history of the national economy. All this gave him strong party and political positions too.

[28] The change in this respect took place during the fieldwork activities of my generation as well. Despite the institutionalization of fairs in people's art, during field studies as students in Koprivštica in the period between 1979 and 1983, we often initially received refusals with arguments that those were old activities, old ladies' activities that did not interest anyone, that this was not 'cultural', etc. However, the interest of the authorities in the local knowledge often changed the self-evaluation of the interviewee and hence the disposition for a conversation.

Fieldwork Differences

With respect to the above, a new view regarding fieldwork was formed. With ethnographers, fieldwork was more regulated and institutionalized and Stojan Genčev's[29] book was used as a model. Genčev was one of the first opponents in the debate with the folklorists. On this issue, almost nothing systematic was published by folklorists, but it was a key moment in their professional education. The students, organized in a Study Group on Bulgarian Folklore, called 'Ivan D. Šišmanov', were included right after their first year of education in fieldwork student research missions, called 'expeditions'. The materials recorded during the fieldwork were given to the archives of the Institute of Folklore and the recordings were discussed during meetings of the study group where, in fact, the techniques of fieldwork was much more widely discussed than macro-theories about folklore, culture, society and the people.

The main issues that were discussed were the mistakes made in communication between researchers and respondents, on the basis of which some basic principles were established. The *faux pas* were recounted as oral history of the study group and were used as a constant pedagogical example on behalf of the senior researchers for the younger students. Typically, the cases given as representing unsuccessful and inappropriate behaviour were not taken from the ethnographers' circles. Below are two such instructive cases:

- A fellow from the Institute of Folklore talking to a local singer: 'Granny, please sing me a ballad' was given as an example of how not to bring knowledge, classifications, and realities which are not part of the local culture;
- A professor from the same institute wanted to record the mourning of a woman who was known for her skills, but she refused by saying that she did not feel like crying because this was inappropriate as there was not a deceased person in front of her. However, the professor insisted, saying: 'Whether you feel like crying or not, you will cry – I am professor: you will cry.' This was given as an example for a position of authority, which the researcher assumed and the possible abuse of it.[30]

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[29] Genčev 1989; see more details about the style of ethnographers' field study in Asen Balikci's chapter in this volume. With respect to the debate, it is important that Hadžinikolov – being a classical historian, who worked with documents and archives – did not have fieldwork experience. For this reason, that part of the debate was left to Genčev, who was a fieldwork researcher.

[30] The fact that researchers were often perceived as officials, as representatives of the state, of the city of Sofia, etc., even when they did not abuse their position, is supported by the fact that very often we were asked for services or were considered as mediators with the authori-

In this context, sometimes the question about the dialogue was raised: i.e., about the ability of the interviewer to assume the position of the interviewee, to imagine the artificiality of the situation whereby one person only asks questions and the other only responds. This was accompanied by recommendations to allow the interviewed individuals talk about what interested them, to ask questions, to take an active position, to be given answers, to pay attention to their questions because the latter most authentically demonstrate their interests, classification schemes and imagination.

Semi-standardized questionnaires that were prepared in advance were meant to be used only by students who were beginners, but, in practice, they were used even by some research fellows. As the 'expeditions' were in teams – in groups of four to five students, who lived for two weeks in three villages – and the requirement was for holistic coverage of the various aspects of daily life and the different situations, when assigning the tasks, the student was given a range of questions about which he most frequently did not have any idea. Against this background, the questionnaires that were prepared in advance were used as mechanical *aide-memoire*, built upon past experience. Despite this, the usefulness of these questionnaires was more than dubious as they represented more a plan for a monograph on a given topic,[31] than a guide for independent work, and in the course of our professional careers most of us stopped using them.[32]

A very important feature of training was the instruction to search for a more holistic approach – not only recording songs, fairy tales, sagas, legends, and memories, but also capturing the context, the groups, the roles, the time, the cause and the atmosphere surrounding the performance. This is a totally different subject, which cannot be discussed in detail here, but which is related to the understanding of folklore as a performing art – always situated in a concrete situation and environment.

At the same time, the study group discussed – albeit rarely – the status of fieldwork. For instance, Živkov, who was the scientific leader of the group, more than once pointed out that our generation had a different attitude towards fieldwork research in comparison with his generation. His formative years were spent in a village, but when he left the village he distanced himself from its culture. As a young person he took part in the fight against

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ties. For instance, we were asked to 'say to Baj Tosho [a nickname for Todor Živkov] that our pensions are too small', or 'Could you help…for…?' and others.

[31] For instance, 'Spring Rituals', 'Winter Rituals', 'A Wedding', 'A Funeral', etc.

[32] After his stay in France Živkov brought some published manuals for anthropological fieldwork which were discussed in the study group and later used during fieldwork at researchers' personal discretion. Personally, I have presented, used during fieldwork and use now in my teaching activities, Bouquiaux, Thomas 1976 and Cresswell, Godelier 1976: these book were given to me as a gift by Živkov back in 1980.

bourgeois prejudices and religious practices but his imagination remained very limited by so called 'valley consciousness'. To us, who are much more cosmopolitan, he argued, fieldwork is exotic, a sort of an escape from the city, a romantic thrill, a fascination with 'the other' and the different.

More than once, the norm about conducting fieldwork in a single village or in a given group, which was included in the international anthropological tradition, was discussed too. On this issue a firm position was never reached for the following reasons:

- Živkov himself carried in his experience the holism of long fieldwork;
- However, Živkov's theory about folklore as a regional culture allowed and required that the long stay did not take place in one village, but in a given region populated by people with common characteristics – Thracians, Shops, Dobrudzhanians, Tourlaks, etc.;[33]
- At the same time, as we studied 'our own culture' in 'our own language', it was not thought necessary to stay in one place for a long time to study the language.[34]

Conclusion

A cursory overview such as the one presented in this chapter cannot give a detailed picture of the real development of the dispute between ethnography and folklore studies in the period between 1975 and1985, but it provides sufficient grounds for drawing some preliminary conclusions:

1. There is no doubt that ethnography and folklore studies developed under the control of the party and state institutions. The reformers had to develop in parallel a scholarly, party and administrative career.
2. At the same time, the overview presented above demonstrates that it is not convincing when talking about the 'science of the people' in 'people's democracies' to make only grand generalizations and gen-

[33] Personally, over the course of twelve years I was 'in charge of' north-western Bulgaria but I worked with a small group of ten villages with similar speech patterns, economies, technical environments, political contexts, rituals and oral traditions. I was there both with a team from the Institute of Folklore, and alone for both shorter and longer periods of time, including during various festivals and fairs at community, district and regional levels. Some of the aspects of this fieldwork were described in Garnizov 2003.

[34] However, regarding the foreign scholars who carried out fieldwork in Bulgaria, Živkov approved of exactly such an approach and helped them to established contacts in a given village. For instance, he helped Gerald Creed).

eral evaluations.[35] There is a certain time differentiation between the disciplines too.

3. Despite the party and state control, there was relative autonomy of scholarly research. During the period under consideration, ideologically argued debates were led between the institutions and the leaders who represented them, while within the institutions the researchers had relative freedom to carry on with their work as they saw fit.[36]
4. The debates in the country were not isolated and provincial, but they were often in accord with the international scholarly debates without necessarily bearing the same 'labels': for instance, structuralism vs. remnants of evolutionism; Marxism vs. structuralism; universalism vs. relativism; interpretation vs. reconstruction; constructivism vs. positivism.
5. At the same time, some of the key topics of the international professional community were blind spots for the local folklorists and ethnographers – the increased self-reflection on fieldwork and the role of researchers, the writing of culture, social criticism, political defence, the advocacy for minority groups and cultures and others.
6. To a certain degree, the end of this relative autonomy came with the beginning of the so called 'revival process' after which the scholars were put under pressure to argue the preliminary given thesis, namely that the Turks in Bulgaria are not Turks, but kinds of Bulgarians who had forgotten their identity.[37]

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[35] Which is convincingly demonstrated on the basis of Czechoslovakia, Poland, Hungary, DDR in the earlier volume edited by Hann, Sárkány and Skalník (2005).

[36] There was a known case concerning a super correct doctoral candidate at the Institute of Folklore, who – following the institutional authorities – frequently referred in his dissertation bibliography to Marx, Engels, Lenin, Todor Živkov and others. Todor I. Živkov told him: 'Do not bother with these citations, I have quoted enough for all of you.' On this occasion Ilia Iliev correctly notes that there were new expectations – namely that Živkov himself should be quoted by his juniors – which, however, was the case anyway because to his students he was an undisputed scholarly authority. If you look at my articles from that period, you would easily note that I had published without being forced to cite either classic Marxist scholars or Lenin's followers or the official Bulgarian authorities, or my own adviser T. I. Živkov.

[37] Paradoxically, after 1985 room was opened up for another type of criticism of the contemporary Bulgarian society, politics and culture – albeit under control and with certain topics forbidden. What I am referring to is the young scholars' seminars in Primorsko, entitled 'Mythology, Art, Folklore', organized by the Scholarly Union for Art Studies. (I was a one of the organizers of the seminars.) They were conducted under vigilant party and administrative surveillance, and prior to every meeting it was clear in advance what the borderlines of the forbidden were. At the beginning, it was forbidden to directly criticize the party; during the second meeting, the borderlines of the forbidden included only the High Council of the party and its members; during the third meeting, the ban concerned only the Secretary General of the BKP. However, the issue of the 'revival' process remained a 'taboo' until the end of the

The consequences of this debate can be seen even today. After the changes, the majority of scholars in the field of folklore studies took a politically liberal position, a critical scholarly position and, to a large extent, applied their skills in practice when faced with the challenges presented to the individual under conditions of political, economic and social change. Scholars from the field of ethnography oriented themselves more towards studying communities, groups, minorities and their interrelations.

Of course, this distinction is conditional and cannot be applied to all representatives of the professions either in the recent past or today in so far as some ethnographers[38] shared some of the folklorists' views even back in the 1970s. The opposite is true also – many folklorists continued to work along the lines of literary or ethnographical traditions. And in the end, the problem does not lie in the differences between the traditions with which we ourselves associate and recognize, in so far as this is a question of our free choice. The problem is that in the public sphere today, there is, in effect, no methodological debate about the disciplines among the different approaches, regardless of whether they are called ethnography, folklore studies, ethnology, or social or cultural anthropology.[39] For this reason, the result of our free choice may turn out to be publicly unproductive, and even useless.

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existence of the People's Republic of Bulgaria as a people's democracy. It is a fact that ethnographers did not participate in these 'liberal' meetings, as well as that they did not organize a similar forum at the Integrated Centre for History. The only ones (apart from the organizers of the seminar on 'Mythology, Art, Folklore') were the philosophers and the sociologists who organized similar meetings in Gjulečica.

[38] Such as Ganka Mihajlova, Ivanička Georgieva and others. It should be noted that after 'Panev' Law and the election of the new administrative management, the two institutes offered refuge to scholars who opposed their respective new directors. Thus Radost Ivanova, Milena Benovska, Stanka Janeva and Anatol Ančev joined the Institute of Ethnography, while colleagues from the Institute of Ethnography moved to the Institute of Folklore.

[39] From time to time, some petty public quarrels could be heard, but there is circumstantial evidence that some serious cultural fights are being held. Thus in 2006, the Scientific Council of the Institute of Folklore made a decision to rename itself to an Institute for Anthropology and Folklore – a decision that did not materialize because, at BAS, the leadership of the Institute of Ethnography (supported by the lobby of historians at BAS) opposed it.

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Zakon za vremenno vãveždane na njakoi dopãlnitelni iziskvanija kãm členovete na rãkovodstvata na naučnite organizacii i Visšata atestacionna komisija. *Dãržaven Vestnik* 104 (24 December 1992).
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Chapter 9
The 'Bulgarian Ethnography' of the Bulgarian Academy of Sciences: Some Critical Comments

Asen Balikci

The objective of this paper is to present a brief analysis of a *magnum opus*, the authoritative three-volume collection *Etnografija na Balgarija* [*Ethnography of Bulgaria*] published between 1980 and 1985 by the Bulgarian Academy of Sciences. It represents the official synthesis that covers a wide range of subjects from ethnology and folkloristics. Under the general editorship of academician Veselin Hadžinikolov (Hadžinikolov *et al*. 1980), the first volume contains a substantial theoretical–methodological introduction plus chapters on Bulgarian ethnogenesis, demography, physical anthropology, psychology and two essays on traditional social organization and customary law. The second volume (Georgiev *et al*. 1983) is devoted to material culture, and the third (Genčev *et al*. 1985), to spiritual culture (world-view, customs, rituals and beliefs, folklore). Clearly, the volumes intend to give a global picture of Bulgarian ethnographic production up to the time of publication. My aim is to specify the theoretical school of thought to which the contributions belong and to examine their ideological content.

From the beginning Hadžinikolov asserts that ethnography is a science, one of the many disciplines coming under the umbrella of history. Ethnography uses its own specific sources and applies its own research methods with the aim of studying the historic development of ethnic entities, national cultures and lifestyles (1980: 57). Ethnography as practical history should clarify the ethnogenesis of large and small ethnic entities and search for the historical roots of specific customs, beliefs or cultural complexities. Obviously general ethnography has links with folklore, sociology, demography, physical anthropology, and linguistics but these links should in no way cloud its specific historical vocation. Marxist dialectical materialism should form the theoretical framework for contemporary Bulgarian ethnography. Indeed notions drawn from historical materialism inspire many analytic parts of the collection, especially when generalizations and evolutionary sequences are made.

In defining the subject matter of ethnography and its basic analytic units, Hadžinikolov regularly refers to the Soviet ethnographer Julian Bromlej and his notion of ethnos. From this somewhat narrow perspective a critique is presented of Western anthropological theories. Boas, Kroeber and Lowie are considered to be diffusionists, followers of the *Kulturkreislehre*. Malinowski is mentioned as a theoretician and defender of British colonial rule. As for the Culture and Personality School, its hidden aim is to demonstrate the superiority of the American way of life and to justify the subordination of lower groups. Durkheim and Levi-Strauss are criticized for their idealistic, mentalist theories, while cultural relativism (Herskowitz) should be rejected for its refusal to apply the evolutionist notion of progress. However, authors such as Julian Steward, Paul Bohannan, Marvin Harris and Morton Fried, it is argued, should be considered seriously since they are followers of Marxism–Leninism (1980: 19). It should be noted that all general theoretical elaborations and criticisms in this Bulgarian book are drawn directly from Soviet sources, although there are references to the original literature.

Following the Soviet ethnographer Julian Bromlej and his concept of ethnos (Bromlej 1977), Hadžinikolov makes the ethnogenesis of the Bulgarian ethnos a primary research objective; 'Macedonians' are consistently considered as part of the Bulgarian ethnos. In an erudite chapter three, the following ethnic strains are assumed to have contributed to the formation of the Bulgarian ethnos:

1. Turko-Mongolic Proto-Bulgarians who established the Bulgarian state in the year 681;
2. partly hellenized and romanized Thraco-Illyrians;
3. the numerous Slavs who are responsible for the core elements of traditional Bulgarian peasant culture.

The synthesis of these three strains begins in the sixth and seventh centuries, and is completed by the end of the ninth century with the adoption of Orthodox Christianity and the Cyrillic alphabet. By the tenth century, the Bulgarian ethnos is fully established as a stable socio-cultural formation. In approaching the next millennium Hadžinikolov largely abandons the perspective of cultural history and enters the polemics of political history. The destiny of the Bulgarian ethnos is considered parallel to the vicissitudes of the Bulgarian state. The Bulgarian ethnos and state are presented as passive political forces, reacting to aggressive attempts of hellenization and islamization.

At the end, however, the Bulgarian ethnos emerges as victorious by virtue of its sheer survival capacity. This historic context and the conception of the Bulgarian ethnos as an extremely stable formation naturally precludes

an evaluation of the large number of Greek and Turkish culture traits which must have penetrated Bulgarian village communities at one time or another. Further, the Bulgarian ethnos is considered to be a highly original historic formation, somewhat self-contained, which excludes the need for comparisons with neighbouring cultures as is more common in a historically oriented regional cultural approach. This ontogenetic conception of the Bulgarian ethnos is in harmony with the critique and rejection of diffusionism as stated earlier.

The analysis of the Bulgarian National Revival movement is interesting. This movement emerges at the beginning of the nineteenth century (and possibly earlier) in the context of rapidly developing capitalist relationships replacing the previous feudal forms. Here the notion of ethnos is abandoned and replaced by the concept of nation. Expanding production and accumulation, and intensified trade within the Balkans and beyond, led to the embryonic formation of a Bulgarian bourgeoisie, which became active in the struggle for national liberation from the Ottomans. The bourgeoisie led, and the masses followed in an increasingly assertive manner. The development of a common literary language comprehended by everybody, the appearance of popular publications in that language and the emergence of preoccupations and elaborations related to Bulgarian history, all led to the constitution of a strong Bulgarian national consciousness. This conscious identity placed stress on its Slavic roots and the brotherhood of Slavic nations. With the founding of the new Bulgarian state in 1878 capitalist relations were firmly established and at the same time class struggles intensified. Against all odds the Bulgarian Communist Party led the proletariat directly to the revolution of 9 September 1944. The communist state became the benevolent sponsor of the magnificent Bulgarian culture under socialism. The meaning of history is clear; the sequence feudalism (Ottoman slavery), capitalism (class exploitation), socialism ('golden age') provides the framework for understanding the final liberation of the masses. The progress implied in the sequence is logical, necessary and inevitable, and represents essentially a local autonomous development, although foreign help from tsarist Russia and the Soviet Union is duly acknowledged. It is this local developmental sequence, heavy with value judgments and a slavophilia centred in the far north, with which Bulgarian ethnographers are invited to labour.

The application of the ideological programme was multi-faceted:

First, the pedagogical task of ethnography was to instil socialist patriotism and national pride (1980: 6). Through ethnographic knowledge the masses (and essentially the young) were expected to develop affection and admiration for the past of their fatherland and to resist foreign reactionary

and national nihilistic influences. In the context of the developed socialist society ethnography was to play a key role in the patriotic education of the masses.

Second, it was extremely important to proceed with a sort of 'cleansing' of ethnographic and folkloric materials. These, as vestiges of the past, often contained elements of religion and a religious world-view of a people living in darkness. Ethnographers were requested to remove these 'parasitic' traits from the folk rituals, festivities, and beliefs, and render to them their 'rational' meaning. The theoretical point behind these recommendations was the rejection of any 'natural' links between ethnicity (*narodnost*) and religion. All references to religion had to be removed from the ethnographic record; ethnography should be made clean and 'rational'.

Third, ethnography was expected to contribute to the struggle against reactionary and 'evil' vestiges of the past, such as gender inequality found among certain ethnic groups (ethnic Turks?). Ethnography should help with the 'optimization' of inter-ethnic relations in the country (1980: 7). Such a statement, I suggest, should be read with reference to the second 'revival process' that took place in Bulgaria in the 1980s, which aimed to assimilate the Muslims living in Bulgaria.

Fourth, ethnography was invited to contribute to the design of new 'socialist' rituals in relation, for example, to name-giving ceremonies, marriages and funerals. The scenarios should keep a few traditional elements within an invented form and should be applied throughout the nation to the detriment of local variation. This, I believe, represents a death sentence to the very traditional folk rituals that ethnographers were asked to define.

Fifth, it was hoped that ethnography would contribute to the development of a 'socialist, proletarian and democratic internationalism'. This could be achieved only after the evolutionist sequencing was applied to other nations, leading to feelings of closeness and brotherhood amongst nations.

Sixth, following party directives, ethnography should play an active role in the aesthetic and moral education of the masses. Elements of 'cleansed' and redesigned traditional culture must be incorporated into contemporary socialist lifestyle. The beauty of traditional material culture should be visible in socialist homes. Traditional humanism and morality, existing in the depths of people's minds, could be used as a first step in the teaching of progressive socialist ethics.

The topical contributions in the three volumes are usually of a very high professional standard and cover many important aspects of traditional Bulgarian culture. Folklore (ritual and intellectual culture) and material culture were given maximum space somewhat to the detriment of social organization. Historicism, (looking for roots) in the context of the evolution-

ary sequence in its local Bulgarian version, is applied systematically throughout. Typically, articles begin with a quotation from Marx and Engels, then provide a reference or two from Bromlej and continue with a serious survey of Bulgarian sources. Strangely, a holistic, integrated portrait of traditional Bulgarian culture at the village level for a particular historical period is not provided. This is probably due to the segmentation of the record according to the standard ethnographic rubrics.

Further, I expected to find a more thorough description of the 'golden age' and socialist lifestyle where a synchronic approach could have been used to describe real actors coping with real life conditions. But such a description is not provided. We can better understand this omission with a quotation from Daniel Bertaux: 'Of all the State secrets in the keeping of communist regimes, one of the closest guarded was undoubtedly the nature of everyday life, its practical contexts, its ground rules and its long-term effects' (Bertaux and Malysheva 1994: 238). Another omission is that almost no space is devoted to the numerous minorities inhabiting Bulgaria: ethnic Turks, Pomaks, Gypsies and others. The Pomaks are briefly mentioned as forcefully islamized Bulgarians (a process which occurred mainly in the sixteenth century). The ethnic Turks are present in a strange discussion of racial types in Bulgaria. Hadzinikolov argues that although the physical characteristics of the Bulgarians and ethnic Turks are basically similar, the rate of blondes among the latter happens to be much higher. This is explained by the prevalence of Slavic racial traits; ethnic Turks are islamized Slavs. This opens the way for the second Revival Movement of 1989 leading to the assimilation of the Muslim populations.

Finally, one should mention an interesting article on field methods and techniques practiced in Bulgaria. Field investigations seem to be of short duration, a few days or a few weeks. A team of several investigators is recommended when complex issues are to be researched. Further, the area approach is recommended, with ethnographers covering a multiplicity of villages. The community study method is not mentioned.

It is clear that in the Bulgarian case Marxist–Leninist theory and methodology, the rigid slavophilism and certain peculiarities of field methodology did not constitute a favourable setting for the development of the social anthropological discipline. Political constraints resulted in the exclusion of the national minorities from the ethnographic record and the almost total absence of references to modern Western authors. My impression is that in these three volumes Bulgarian ethnography emerges as an isolated strain, certainly looking north to Slavic Russia for guidance but more often preoccupied with local historical issues.

Nevertheless, my focus on the ideological and political constraints should in no way prejudice the descriptive ethnographic record, which, in my opinion at least, remains admirable.

Considering the importance of research methods in the historic development of ethnography as an autonomous discipline I would like to mention a manual published in 1989 by the eminent ethnologist Stojan Genčev entitled *Field Investigations in Ethnography* (Genčev 1989). This manual summarizes previous publications on the topic and it is admitted that the methods prescribed and the points of view expressed have had a deep influence on the discipline of ethnography in the country. The ideological orientation of this manual is in full harmony with the prevalent socialist ideology of the time.

First, right from the beginning it becomes apparent that this manual is about Bulgarian national ethnography concerned with Bulgarian ethnic culture, presumably of Slavic origin.

There is no clear statement about the existence of other ethnic cultures in Bulgaria although at one point confessional differences are mentioned. Further, it is assumed that Bulgarian culture should be studied exclusively by Bulgarian field ethnographers, who have already a general knowledge of the culture. The Bulgarian ethnographer should be capable of easily correlating the specifically local, village customs to the broader picture of the general Bulgarian culture with which he is familiar. The possibility of foreign, let's say Western, investigators doing fieldwork in Bulgaria or Bulgarian ethnographers working in African villages is totally absent. This nationalistic orientation is further visible in the bibliography where only two Western publications are mentioned, namely the *Columbia Handbook on Method in Cultural Anthropology* and Leach's *Culture and Communication* (in Serbo-Croatian).

The positivistic approach is expressed with strength throughout the manual. Culture, cultural elements are very real, hard facts one can observe and study right in front. Culture has a four-part structure:

- A – Material culture (subsistence, dwellings, crafts);
- B – Spiritual culture (beliefs, rituals and festivities);
- C – Social culture (kinship, local organizations);
- D – Artistic creations (songs, dances, music).

The author (Genčev 1989) elaborates on this structure by adding a great number of thematic subdivisions always with reference to Bulgarian materials. Cultural traits are studied separately, in distinct niches. 'Religion' as a distinct category is absent. The manual concentrates almost exclusively on traditional culture for the period from the end of the nineteenth century to the middle of the twentieth. It should be noted that the author avoids using the

term 'traditional culture' preferring 'people's (narodna) or national culture'. In this he follows closely J. Bromlej's conception of traditional culture.

The whole issue of socio-cultural change imposed by early local capitalism and later by vigorous socialism is absent. Basically this is a manual for what is described in the West as salvage anthropology expressive of a Boasian drama: the traditional culture as an original, historic creation is disappearing fast, let's rush and save what we can using the methods described here…

The basic aim of Bulgarian ethnographers should be the constitution of ethnographic archives, stores of original ethnographic data collected in the field following the application of specific methodologies: participant observation, interview and above all the use of questionnaires!

The preparation of specific and detailed questionnaires is an extremely important task, taking place during preliminary, exploratory fieldwork over a wider area. This may seem rather odd to a Western investigator, yet it isn't necessarily so. The aim is the constitution of ethnographic archives, and questionnaires certainly can help us in this task. With a questionnaire in hand the ethnographer cannot miss some discrete culture elements, the questionnaire will provide us with all the thematic niches we need to fill up.

Unfortunately there are very few ethnographic examples in this manual. The language remains abstract with one exception, the institution of 'godparenthood' (kumstvo) in Bulgaria, recorded and analysed by the author and presented in the manual as an ethnographic case study involving the use of a multiplicity of field methods. We suddenly realize that salvage anthropology can still be a most important field in Bulgarian ethnology.

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Chapter 10
Politics – Gender – Habitus: Anthropology as a Profession

Anelia Kasabova

In an academic discipline, the subject matter of which is the variability of human forms and systems of interpretation, and which is aware of its own power of definition, it is even more necessary to analyse one's own manners of perception, ways of action, and social forms of organization.[1] A consistent historical–anthropological history of science, apart from a classification and delimitation from various scientific discourses, requires permanent reflection of one's own research practice. In order to understand objects of research and theoretical instruments, one must also try to 'reflect those factors outside the confined (written) scientific discourse which exert a certain influence on one's own scientific practice' (Dressel 1999: 249; see also Dressel and Langreiter 2002).

The title of the conference on which this anthology is based was 'Doing Anthropology'. This requires the application of a principle made by Pierre Nora: 'The practice consists in examining one's own history the way one would the history of someone else' (Nora 1987: 9).

My contribution focuses on six life-history interviews conducted with Bulgarian scientists – three men and three women of different generations and social backgrounds, who all work in the field of ethnography/ethnology and whose professional career took place, or at least started during socialism. An application of the principle of self-reflection, however, means that one must not only apply the attempted objectifications to the person interviewed, but also to oneself. Therefore, I will explicitly incorporate myself into my attempt at an analysis in order to address the significance of the categories of gender, generation, social background and positioning in the scientific field, and to demonstrate through an everyday and experiential perspective the

[1] The corpus of literature on topic is extensive. See for instance: Jeggle 1984; Medick 1989; Bourdieu 1993 in Berg and Fuchs 1993.

disintegration of the conventional dualities of personal and political, of public and private, of science and politics, and of science and gender.[2]

To emphasize the fact that this study is not about the personal histories of individual scientists, but about fundamental and supra-individual questions, about structures and the like, all cases have been made anonymous.[3]

Science as a Political Event

The first scientist – M1 – born in 1930 into a poor peasant family, experienced the early years of socialism as a democratization and an opening up of the school and educational system to children of the lower classes. Grammar schools were extended to blanket coverage and provided the, until then, uneducated classes with chances for advancement:

> I started grammar school in 1945. I had finished village primary school, then went to grammar school. And that was due to the New Times [the socialist age after 1944] arriving in the village, so that all of us farmers' children could study in grammar school. We were sent food parcels. That was the time when everybody left the villages for the cities. My village was deserted. Of 2,000 people, there were only 300 left.

But material–financial difficulties prevented him from attending university: 'I wanted to go to university and applied with all my documents. But that was still private property. My father had no money at all to support me financially. We were five children and he was already sixty years old, born in 1890. "I", so he told me, "am a pensioner now, I don't have any money, I cannot fund you. I can buy you a ticket to Sofia." … This is why I became a teacher in the village for one year, at the local grammar school.'

His graduation from the military-political school 'J. Stalin', founded three years previously, in the year 1951 after a two-year course M1 explains as a result of financial problems, of his own difficulties in visiting a university and of pressure on the part of the party committee. He neither denies his left-wing Marxist views nor his own political activities – in the Comsomol organization of his school as well as in the Communist Party, which he joined during military school. In 1956 he abandoned his officer's career – which was presented as resistance and struggle against the leadership, who

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[2] The questions spring from the multilateral project 'Reflexive historische Anthropologie' (Reflexive Historical Anthropology). I was asked to join in this project by Gert Dressel and Nicola Langreiter and conducted life-history interviews with Bulgarian scholars. I would like to thank Nikola Langreiter and Gert Dressel for their inspiring cooperation.

[3] We also followed this principle in our publication Dressel, Kasabova and Langreiter 2005. However, without our knowledge and consent, the editor replaced the codifications with initials.

threatened him with expulsion from the party – and he enrolled for Bulgarian Philology at the university.

His interest for ethnography M1 describes as 'accidental' and pragmatic: in his last year at university, he met a scientist who worked at the Institute of Ethnography, and she recommended that he apply for a position at the institute:

> We had not heard about that institute. Folklore – that we knew, but ethnography, what exactly it deals with … She explained to us: 'Go and speak with Romanska [a Slavist, daughter of the first head of the Institute of Folklore 1947–58, who was at the time a lecturer at the university and became head of the Section of Folklore from 1962 to 1969]. Take an interest in your villages, what they do there, the customs, the traditions, this way you can defend your dissertation easier and don't have to take the state exams.' And that aroused our interest, instead of two state exams in Bulgarian Language and Literature – one written, one oral. That was a horrible thing!

After two years of application at the institute, M1 achieved a position as trainee/research assistant. In 1963, a party organization was established at the institute, the secretary of which M1 became.

At that time – in the early 1960s – the so-called 'purgings' of the 'Stalin era' (after the 5th party congress in 1948) had largely been completed in the scientific area; special edicts, particularly in the field of humanities, served to replace scholars accused of 'harmful activities' or bourgeois ideology with party loyal cadres (frequently grammar-school teachers or graduates of Soviet party institutes).[4] The humanities, i.e. the so-called social sciences, were subjected to a restructuring due to their legitimation function as early as the 1940s and 1950s. This reform completed the amalgamation of previously separate and autonomous academic structures under the umbrella of the Academy of Sciences as well as – based on the Soviet model – the institutional division of teaching and research (university and academy). Through the foundation of the Committee for Science, Art and Culture, all scientific and cultural events were now under control of the Communist Party. The aim of the centralization was to secure governance and control over science.[5]

M1 remembers the early days of his career at the institute in the following way:

> I was assigned the task … I was called and told: 'This institute is in a difficult condition, the staff consist of a more or less coincidental se-

[4] Mutafčieva 1995: 12–18. Cf. Milena Benovska's chapter in this volume.

[5] Markova 1980: 151. For the institutional development and the transition from philology to history, see the timelines.

> lection of people. In short – it is not even a proper institute yet.' In past times there was only a museum … I have spent – and I am not saying this by chance – fifteen years fighting and arguing here.

Life-historical memories start from the respective present, they are retrospective constructions linked by complex causes and effects. Necessarily they are selective, true to values and future oriented. Undoubtedly the present discourse on state socialism generates a strong pressure to justify and has a structuring effect on life-historical memories. Although the interpretations and constructions of the history of the socialist era do not find general consensus, but are in part controversial and result in clear horizontal societal splits, in all biographical texts we can detect a distancing from the leading state and party elites. Striking in M1's description of the 1960s is the emphasis on his own resistance and his opposition to deficiencies within academic life and the party. He gives priority to the political constellations, the commotion of interrelations and clashes with the party apparatus, which from today's standpoint he views as a loss of intellectuality:

> I realized that this organization political work … how do I put it … that this work was fruitless. It doesn't produce anything, doesn't develop anything, neither a profession nor anything else. Tomorrow, if your boss is in a bad mood or annoyed with you, you might be fired. That is a kind of work that makes you faceless. That I understood.

M1 does not deny his own sympathies for socialist ideas on society; neither does he renounce his political party career. He places emphasis, however, on his massive problems with conservative party cadres, his efforts for a democratization of the structures and against persecution by the party, as well as his contribution to a 'scientification' of science by way of the introduction of new issues and methods and an 'opening up' of ethnography by way of different forms of recruitment of junior researchers. Even if M1 associates himself with a generation who made a new start in science, he regards his own research and that of his generation as 'halfway to a grand science', and himself as 'not a (great) scientist'. This he explains by his background and education, by his lack of language skills and by the isolation from 'Western' science. 'We coped with the Russian language somehow, but it is not enough for scientific work.'

Research Issues and Taboos[6]

Orderliness and a coordinated central management with concise thematic parameters became the basic principles of academic work in socialist Bul-

[6] Since research theories and approaches are the focus of the chapters of Milena Benovska, Vasil Garnizov and Ilia Iliev, I will only briefly mention some problems.

garia.[7] In ethnography, the approaches of research of the pre-socialist period were maintained. Furthermore – but not exclusively – the model of an ethnic-cultural nation-state determined folkloristic research.

What was explored above all was 'the village'. Peasant culture was mythologized as a bearer of continuity from antiquity through the Middle Ages. This field of study has its beginnings in the late eighteenth and early nineteenth centuries and is closely connected to the process of nation building and the emergence of the Bulgarian state. As in every country where folklore was able to take root as an area of research, this process was triggered by an intellectual and political elite: the ethnographers/folklorists in principle belonged to the group they investigated, while at the same time they were part of the social stratum that considered it their task to explore, consolidate, defend and often also invent the 'matter' of this same group. Parallel to the type of the folklore theorist, the type of the (village) field researcher developed, who due to his social background had a close relation to rural culture.[8] For both types of scholars the intertwining of scientific work and national economic and socio-political commitment was characteristic: the collection and remembrance of cultural and other documents of the 'common' past of an 'imaginary society', which also claimed territorial continuity; the propaganda and the charging of national symbols with sacred feelings, their sacralization so that 'the people' would identify with the national programme (Kasabova 2000: 94; Kasabova 2002).

This scientific approach was maintained after the communist takeover and was combined with positivist methods of the collection and conservation of fragments of past folklore. The merits of this work are the completion of archives and folkloristic museums or ethnographic departments within the country's historical museums. In the 1960s, a long-term research project was started, the results of which are presented in a series of regional studies: based on a uniform national model of culture, specific regional features were described and national atlases prepared. The collected materials were classified according to regions or ethnographic groups (only of the majority population, however, minority groups were not recorded), and were organized in subject areas such as 'traditional material culture' (economy, folkloristic architecture, costumes), 'traditional spiritual culture' (world-view, folk medicine, calendar customs, family customs, work customs), dialects, etc.

The overvaluation of the 'old peasant culture' was – similar to the pre-socialist period – not directly connected to social background. After 1944,

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[7] These principles can retrospectively be assessed positively or negatively, according to the respective observer's point of view and depending on his or her present project participation.

[8] For the problem of the 'double insider syndrome', see Naumović 1998; Benovska-Sabkova 2000; Kasabova 2000.

several of the new scientists grew up in villages – in our case, two of the interviewees, M1 and X1. This is how M1 describes the closeness in his fieldwork:

> I met my own people and their culture with all my respect; I was touched by the folk songs we sang together. There are a lot of memories. I hold the culture, the customs and everything in high esteem. For me, this is not the 'simple/stupid people', but 'my people'. I come from these village classes; I explore my people and the milieu I originated in. I don't go there to watch the 'simple masses', something distant from myself.

The concept of peasant folk culture research was also supported by researchers of urban origin – although many of them in the first generation.

In contrast to pre-socialist times, during which research activity sprang from a strong national–political commitment, cultural studies after 1944 were regarded as a 'niche' to escape from ideological pressure. A quote of M1: 'Our ethnographic research was not subordinated to any kind of political situation. Our science was a culturological one and we studied the processes of culture … It was purely cultural processes we investigated …' This view can be supported by several examples.

This is precisely because of its perception as a refuge from ideological constraints, a point of view which is actually ahistorical in nature was able to survive until the present day.

Clearly detectable is a pronounced tendency to archaize, which can also be inferred from the interviews with members of this generation of researchers; they used default collection plans to search for elderly informants in order to interview them about what used to be believed, known, done in olden times – and not about the present day. The principal methods for the retrospective reconstruction were questionnaires and official surveys.

As the head of several regional studies, M1 describes how he attempted to motivate the collectives:

> I told them, 'Our generation is halfway to a grand science, but the regional studies will be a monument for our generation for years to come. Because you are the last persons who can still collect traditional data in a comparatively systematic way. These data are lost quickly.'

In this representative/static approach, in which cultural elements were considered an expression of a relatively consistent 'group mentality', economic, social and political transformation went largely unnoticed. This, however, might easily overlook the fact that to take individual cultural elements out of the respective context and to transfer individual pieces of information to the entire group or the entire cultural system is highly political. Today, new

questions are being asked on the basis of sources like the diaries of ethnographers: questions about the collectivization of agriculture, about religiousness in socialist Bulgaria, about the relationship between the researchers and the researched. In the ethnographic diaries, which were to be kept during fieldwork, comments or life-historical accounts – depending on personal opinions – of the so-called 'informants', which had been given parallel to the formal interview answers, were noted down (or sometimes they weren't) (Bogdanova 2004, 2006).

Certain shiftings in the thematic parameters took place over time. In the 1950s and 1960s, the ethnographers were assigned the task of devoting more attention to questions of ethnogenesis, in cooperation with historical disciplines, in order to support the idea of a Slavic community of descent. When in the late 1960s and early 1970s (particularly after the 10th party congress of the BKP in 1971) nationalistic attitudes intensified, the Thracian heritage was stressed as an antithesis to the prevalent over-emphasis on the Slavic linguistic and cultural community. This also had institutional consequences – the Institute of Thracology was founded in the early 1970s. Corresponding to the renewed and increased legitimation demands with regard to folklore, the Institute of Ethnography was divided in 1972/3 and a second, thematically and methodically related department was established – the Institute of Folklore.[9]

The academic developments can be reflected in their political structurings. At the same time, the life histories of researchers show other facets of academic practice as well – the significance of inter-personal relationships, of power ambitions and career opportunities. After the separation of the two institutes, M1 stayed at the Institute of Ethnography, although he used to work in the folklore section; the reasons were a quarrel with one of his colleagues who had ambitions of becoming director of the new institute, and the fact that he considered the prospects of promotion for himself to be better at the Institute of Folklore. Just prior to the institutional separation, he founded a new section, the head of which he became: the section of 'Ethnography of the Present'.

This research approach M1 developed thanks to his belonging to the 'travel cadre'. After the socialist takeover, contacts with 'Western' countries were largely disrupted on the grounds that 'science and art are not firmly scientifically established in the capitalist countries, therefore the experience to be gained from this would actually be a negative one'.[10] Instead, relations to the academic institutions of the socialist countries, and above all to those of the USSR, were built up systematically.

[9] See the timelines in the Appendix; for the theoretical debates, cf. Garnizov, this volume.
[10] CDIA, f.143, a.e.149, I.13 – quoted in Kalinova 1999: 228. Also see Roth 2005.

However, the possibilities of official trips were limited even within the socialist system. M1 developed his insights on an 'Ethnography of the Present' thanks to his international participations and contacts:

> Back then I went to a conference – it must have been in 1972 – in Czechoslovakia. There were scientists from all socialist countries, something to do with the present, I don't remember the precise topic. And I proposed: 'Why don't we unite our efforts to study the present, why don't we conduct investigations together, etc.' So, we founded an international committee on the study of the socialist present. I was elected and remained chairman of this committee for four or six years.

The field of study M1 is talking about in the first person in his interview started to develop from the mid 1960s onwards.

In cooperation with the ethnographic institutes of other socialist states, international research projects were realized. There were attempts at a discussion of new problem areas in ethnography – particularly of the transformation of traditional culture into the culture of the present as well as of current societal processes: industrialization, urbanization, agrarian cooperation.

Some of these studies remained unpublished – as doctoral theses or in archives.[11] Although these works were based on the construct of a 'traditional culture' and searched for 'remnants' and relics, it was a still an approach to the analysis of contemporary social processes that was relevant to present times. The opportunities for contemporary research of socialist everyday life were, however, limited by strict party and state control.[12] What also remained marginal and did not result in a fundamental paradigm shift were the historical–anthropological studies, in which various types of sources were evaluated with a socially more differentiated view on everyday phenomena and with a more complex methodology: demographic and economic statistics, legal sources, press materials and memoirs.[13]

The leading studies and theoretical debates were those with a pronounced pragmatic aspect of 'social engineering', which aimed at the mod-

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[11] For instance the study of Ivanička Georgieva (1964).

[12] It was not only 'insiders' who would not transgress the unofficial borderlines – 'outsider' scientists, who themselves were subject to rigid rules and regulations, but whose research opportunities were at least expanded in the socialist states from the late 1960s on, still avoided issues and problems critical of the system. See also Roth 2005.

[13] A striking representative of historical anthropology is Georgi Georgiev (1979), whose work opens up new problem areas: the development of a new view of the world and oneself; the evolution of family structures and of the relations within family structures; changes in the culture of eating and amusement; etc. Even if this did not conflict with the maxims of 'Marxism–Leninism', the image of a dynamic era is sketched out. Cf. Brunnbauer 1999.

ernization of life and the creation of a socialist everyday culture.[14] Convinced of the ideals of socialism, M1 also played an active part in the 1970s debates about a necessary modernization and incorporation of the traditional heritage into the socialist system of holidays and rituals (Pimpirewa 2005).

Parallel to that a new line of research and interpretation developed during the 1970s, inspired by French structuralism and imparted frequently by Soviet semiotics.[15] F1 – thanks to research stays in the USSR and USA, one of the first to develop these approaches and to apply them in Bulgaria – remembers the early days as a time of hard discussions, requiring a lot of courage and assertiveness. At the same time, her self-portrayal calls attention to the problem that resistance to the 'mainstream' within a discipline is only advisable after achieving a certain position in the academic system: a detachment in terms of content took place for F1 only after she received her Ph.D. and was successfully integrated institutionally. Emphasizing the marginality of contents in many contexts aims at an accentuation of one's own creativity and potential for innovation.[16]

At the same time, different strategies of integration and conformity became apparent: 'I felt compelled to delete certain Western names or to quote them via the modern Soviet authors. What scientific acrobatics I performed, because I was accused simply for quoting Mircea Eliade, for example!' (F1: 15–16). 'Marginality both on content and on an institutional level hardly is promising.'[17] The study of symbolic and mythological questions, which in the 1960s had not been approved without difficulty, advanced during the 1980s to become the dominant and leading research paradigm. Following Levi-Strauss, we (I am referring to myself, too) classified our 'traditional culture' as a *société froide* (cold culture), which due to its construed closed character was said to have preserved remnants of older cultural epochs. The predominant terms were 'continuity', 'custom', 'ancient', etc. The peripheral positioning in many self-perceptions and self-portrayals remains, even if this 'marginality' was based on collective attitudes and had in fact become 'mainstream'.

Time also facilitated this. In general, starting in the early 1970s and continuing into the 1980s, an eclecticism of methods and theories was gradually permitted and prevailed. Some intense debates[18] took place, which

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[14] See Ilia Iliev in this volume.

[15] Find more about the content discussions in Vasil Garnizov's chapter in this volume.

[16] Dressel and Langreiter show examples of Austrian and German scientists, see Dressel and Langreiter 2002.

[17] Dressel and Langreiter 2002: 143. For the 'consecrated heretics', see Bourdieu 1988.

[18] Cf. the chapters of Vasil Garnizov and Milena Benovska-Sabkova in this volume.

largely remained restricted to the framework of national science.[19] This way, opportunities for distinction were offered so that everybody could find their niche. The discussions made intellectuals feel free in a certain way, and at the same time they strengthened the perception of ethnographic folk cultural studies as a refuge from political pressure. And this was exactly what distracted attention from taboo issues like everyday life, social institutions, power relations, material conditions or minorities.

Science as a Social and Gendered Event

One of the taboo issues was the question of women's rights. Before I turn to the problem of gender aspects of academic practice, I would like briefly to address the question of the networks in science as a 'social event' (Dressel and Langreiter 2002: 138; Dressel 2003: 209).

In his life-historical recollections M1 argues and accentuates the horizontal networks and stresses the lack of teacher–student relationships: 'I can honestly say that nobody taught us, I was no one's disciple, I had no scientific tutor. We were research assistants, not doctoral candidates.' He only mentions one 'great scientist', a male historian and head of the institute, although he had originally been made aware of the institute's existence by a female researcher, and although at that time women indeed were working in this field of science, at the institute as well as at university.

The collectives of thought and discussion M1 talks about and portrays as rewarding are distinctly gender specifically male dominated. These horizontal networks serve as information and communication networks, which in academic life are of enormous importance. At the same time they are protective and supportive networks with regard to career advancement, even if they resulted not only in cooperation, but also in competition and oppositions, and if, in retrospect, delimitations dominated.

One's own male identity and, generally, gender are only rarely reflective categories in life histories of the scholars. One ethnographer relates:

> … Ours was a great class. Now that I look back, five or six of us became professors. T. S., N. G., Z. T., the one who has become famous in Paris [all male names]. He was also in our class and in our department. There was another one who worked in the library … We are seven or eight professors.

To the interviewer's question 'Only men?' he answers:

[19] The almost complete lack of comparison is in part due to limited financial means and in part to the concept of folk cultural research and the focus on 'one's own culture' connected to it. For more detail, see Kasabova 2002.

> No. 80 per cent of the subject 'Bulgarian Philology' consisted of women. There were 120 students, about 30 men and 80 women. The women were so … ambitious and had better marks, too. Some even graduated with a 'fully excellent'. But … well, they became good teachers. The boys could have an overall mark of 5.00 or 5.5 (corresponding to a *cum laude* or a British Lower Second-Class Honours) – for a boy that was a good mark – but they usually had broader interests. I can't remember now … Ah, yes, there were women who did scientific work. In the cultural field of Old Bulgarian Studies we had one or two, at university there were one or two assistants or lecturers. Yes indeed, there are women. But many from our discipline later went to work in editorial departments, for newspapers.

Again asked by the interviewer: 'How about your family, since you both occupy yourselves with science?', he talks about his private life:

> You have a friend at your side who shares interests and topics of conversation with you. I couldn't imagine my wife, for instance, being an engineer and not having a clue about my work, then there wouldn't have been anything to talk to her about in a simple everyday conversation. And when we get home, our main topics are, who writes about what, which problems have come up, I found this here, and so on. We also exchange ideas and lead a professional conversation on an everyday level. From a different perspective – you understand that you have a common fate with this person, both have their special interests, but you can help the other with advice or conversation. He can help you. The two of you do something together, this way your fates are tied together even more closely, and even closer gets your spiritual relationship.

Although in comparison to accounts of their professional careers their private lives come second, they do play an important role in the reports of Bulgarian scholars of this generation. Male as well as female scholars talk about their financial and housing problems in socialist Bulgaria, about the difficulty of reconciling job and family.

> And since my study was actually our bedroom and at five o'clock in the morning it is still dark in winter, I took a pen and wrote down my ideas in the dark – if I had any ideas. I didn't want to turn on the light … I might have woken my husband. He worked in the only other room, where the children slept. There he sat and typed until dawn, next to the children. And I in the other little room, which only was ten metres square. There was no room for a desk, so I had squeezed a small kitchen table, an extendable one, into a corner. My books were spread all around me, on shelves, on the floor, on the

> shelves – everything was so crowded that I could not move my chair at all. The room was so confined, the slightest move made me bump into the bed. The entire flat was only 56 square metres, ground floor, with lots of problems – it was very noisy. But there was no other possibility, we had to manage somehow.

M1's description sounds similar:

> For long years I was living in a kitchen. I earned 70 Leva, half of it was spent on the rent. 40 Leva – try to live on that! This kind of poverty is hard to bear and it is difficult to imagine how one can exist under such conditions. But even today the working conditions in science are not normal, they are far below normal. Even today. If you come from the countryside, it is very difficult to gain a foothold here (in Sofia). … I married M in 1967, but we didn't get a flat until 1973. So, for almost ten years I didn't have a flat. They gave us one, which was very small. Whatever. In this flat there was no space to work, because we had a living room, a bedroom and a kitchen. We had a child. It was impossible to work there. Both of us had the same work. We used to work here at the institute.[20] M's grandmother, mother and father came and took care of the child, and we came to work here. All day long we sat here and worked, it was different from today …[21]

M1's descriptions of the task sharing in his family as well as the comparisons to families of neighbours or friends illustrate two aspects:

(1) A continuation of the ideology of 'natural gender characteristics'. His own male participation in the household chores and the equal partnership with his wife-scientist are emphasized:

> When I know that my wife has been working late into the night and she is tired, then I vacuum the house, that is my job. I also go shopping. Of course there are some tasks that are not my job, women's work – I don't know how to cook. Only she cooks. Laundry and ironing are her jobs. She will not let me leave the house in a dirty

[20] There are no individual offices at the Institute of Ethnography. Each department (of 4 to 20 scientists) had one workroom (of 10–20 m²) at their disposal. Research was, and still is, done individually at home; office hours are served at the institute (twice a week for three hours).

[21] Both families in which both spouses were working at scientific institutes were able to buy flats of their own in the 1980s (if only in one of the typical prefabricated buildings). The assessment of the socialist era with regard to economic independence, however, is controversial and depends on the political views and the different opportunities in pre-socialist times.

> shirt, she insists on it. But with all the other chores – we have come to an understanding about who does what.[22]

Noticeable are the description of male housework as 'help' and the gender-specific classifications of various tasks.

(2) An upgrading of the double role of women, which simultaneously means a downgrading of the position of a full-time housewife and of housework in general. M1 dissociates himself from families in which he detects an 'unequal partnership', although it is characteristic that in the examples he gives, the man is always a scholar and the woman is not (but instead works as a shop assistant or is employed in administration). Cases, in which the scholar-man is looked after by his wife, but on an intellectual level the relationship is hierarchical and not based on mutual understanding, are called a 'true tragedy' (M1: 41):

> I observe that – the man is a professor and his wife has no idea of what he does. She cannot really appreciate his work: 'Ah, you sit there all day and drink coffee. And I drudge and go shopping and go to the market. And he just takes his briefcase in the mornings, sits around all day long and comes back home in the evening saying: "Oh, how exhausted I am!"' I know lots of cases like this …

In this description, a low regard for household chores as inferior work and for the role of a full-time housewife become obvious. At the same time, it calls to mind the (old) demand for female education so as to better understand and support one's husband as well as to better fulfil one's 'female duties' as a wife and mother.

Through this comparison, he paints the self-image of an, if not exceptional person, but exceptional man – a scholar and a good, non-patriarchal husband. What he doesn't reflect on, however, is the fact that his wife – who shares the scholarly work with him – in accordance with gender-specific expectations and societal realities, was not able to pursue a career like his own, in order to be able to combine job and household tasks. The dichotomous role assignment certainly is not assumed as conscious discrimination; rather it is taken for granted: the wife-scholar typed out all the works of her husband, while he could make use of working conditions not accorded to his wife-scholar: 'In the 1980s I rented a study near our flat, next to the park. I rented one room and worked there for thirteen years. This was where I wrote my big doctoral thesis (habilitation). Anyway – I left the flat in the morning and, like all the other people, returned in the evening.'

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[22] In a society of deficit, men need to be included in the household tasks to a higher degree, particularly for 'male' jobs – repair work, refurbishing and the like; shopping; as well as the use of technical appliances are often 'male' household chores.

This passage, and also the comparisons to families with only male scholars, is reminiscent of Max Weber's 1917 elaborations on 'the man of science': science as a naturally and unchallenged male profession, although Weber replaces the principle of divine vocation with hard work and the ideas springing from it, which could/should result logically in gender neutrality (Weber 1973 [1917]: 316, 329–30; see also Engler 2000: 122). More than eighty years later, science is still largely male connoted – even though by rather hidden mechanisms. Even in a socialist state, which has officially announced the equality of the sexes and opened up education and science for women by way of a quota system.[23] This leads us to the category of gender.

The Category of Gender

In socialist Bulgaria, communist ideology on the one hand commanded women to be socially active and to pursue a profession, using slogans of 'liberation' and 'equal rights' for women. On the other hand, social reality – low wages/low standard of living – required both partners to have a job. Therefore, the role of a full-time housewife was not only ideologically frowned upon as a 'remnant of bourgeoisie', but was also economically hardly manageable due to material conditions.

From the 1960s on, the emphasis was on the mother role, but simultaneously working full-time. Despite certain attempts at a public discussion of the role of the father, it was still mainly the women who had to reconcile gainful employment with family. The massive propaganda declaring the question of women's rights solved, and the lack of socio-political movements to consistently articulate the necessity of fulfilling the claim for emancipation, led to many internalizations of gender dichotomies. The political and social upgrading of the 'working mother' presented a source of power and worth for women. At the same time, women received recognition for the maintenance of the non-state, private space of home and developed a feeling of individual autonomy through their employment.

The analysis of (historical) statistical data on male and female proportions, including their positions in the hierarchies of various humanities departments, has not been completed yet. A hypothesis, based on a first impression, could be that the 'hard' social sciences (history, sociology) were male dominated and have remained so. In contrast to that, ethnography, folkloristics, archival studies, etc. as disciplines that had been classified and

[23] On the patriarchalism of state socialism, using the example of Bulgaria, see Luleva 2003.

marginalized as 'historical auxiliary disciplines' during socialism, present a high and increasing percentage of women in their entire structures.[24]

Therefore, serious contradictions and actual discrimination against women were by many not perceived consciously. Among the smaller percentages of men are proportionally more professors and persons in leading positions. X1, born in 1938, whose career ended as a lecturer, and X2, born in 1940, who became a professor, do not talk about any perceived discrimination against themselves as women, even though the various problems with men superior to themselves in hierarchy become apparent: permanent dependence on one's academic tutor/supervisor, a lack of attention and even being passed over by the head of the institute when it came to filling positions, temporary unemployment, years of exploitation as technical assistants, disguised plagiarism on the part of the academic tutor, etc. Also obvious in their accounts are the 'strategies' they have to apply in order to make progress – a process of socialization within the academic society that also constitutes a process of adaptation to hierarchical gender relations.[25]

It is characteristic for women to emphasize their activities in formal networks, but there are less indications of informal networking. Women of this generation cannot afford to retreat from committee work, although the additional work, often connected to administrative–technical tasks, leaves them less time for their own research.[26]

A retreat is hardly possible, because women of this generation had to be even more committed in order to get noticed. Furthermore a retreat would also mean a loss of information. Both women's careers were more successful than average (lecturer, professor); and both women in their life-historical

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[24] The percentage of women at the Institute of Ethnography including the museum at present is 70 per cent. Marginalization was often parallel to feminization, although frequently the feminization was, and still is, believed to be the reason for marginalization.

[25] X2 invited the head of the section and simultaneously party secretary of the institute to be her academic tutor in order to speed up the procedure and to avoid him as an examiner (because in that case she might have to wait years for the review); she wrote her professorial dissertation in secret, because another scholar of higher status was jealous and would have claimed to work on the same subject; she complied with the demands of the editor-in-chief so her book was published; etc.

[26] Both researchers worked as academic secretaries to the institute for many years, which for them meant many additional administrative tasks. X2 said: 'After that [after the first rejection], however, they were already successful in recruiting me for this kind of work … Since I was very ambitious, had children and a difficult family situation, had a husband who was a research assistant, I had to work very hard … And I worked like crazy, I got up at five in the morning. Really, like crazy! Until 10 o'clock, I did my own scientific research, after that I went to do the administrative work … And (the head of the institute) had the habit of continually being on sick leave … So I had to do all the work of the head of the institute, not only that of the academic secretary.'

recollections basically share the self-conception of an active, independent personality and of science as a concept of life, as a vocation closely connected to their individual identity (Dressel, Kasabova, and Langreiter 2005). In these cases – as in the men's accounts – an autonomous 'self' finds expression. They want their achievements to be respected independently of their sex and seize the concept of the active personality, in which achievements are made instead of being ascribed. In the field of science, autodidacticism and hard work, the constant search for new things – trying to excel in many scientific areas and being successful – is stressed.

Courage and the will to overcome barriers of various kinds are fundamental traits in the self-conception of female scholars, too. This way, female researchers in their life histories often follow the 'ideal type' – the image of the independent scholar who succeeds through her own achievements. At the same time, X1 repeatedly expresses her regret about not having achieved everything she had wanted to achieve. But this she attributes to her own failures rather than to unfavourable circumstances, to gender relations or even to structural deficiencies of the social system.

It is obvious in the life histories of both female scholars of this generation that the reconciliation of children and profession, the organization of family life and a job that requires frequent travelling, fell to them as women, while their husbands (in both cases scholars as well) mainly felt responsible for the organization of their research activities. It is characteristic that in their academic careers, the women rely on support – particularly in childcare – from their mothers or paid nannies rather than from their husbands.[27]

During the final stage of the preparation of her doctoral thesis, X2 had the opportunity to work at the institute, i.e. outside her flat, and for six or seven months went to work every day. This led to a family crisis and accusations of 'neglecting her duties as a mother and wife':

> That is the tragedy of the female scientist. On the one hand you are a colleague who is neither a party member nor related to any scientists. On the other hand – struggles at home! Struggles with your husband, family, the overall difficult situation. You have to manage and organize everything. You have to organize the childcare, you have to decide with your husband about whether in this particular moment, when you are in a 'creative fever', you need to get divorced. Anyway, I finished my doctoral thesis.

She decided to write her professorial dissertation in secret. Unlike X1, she talks about herself and her husband:

[27] For the role of the grandmother, i.e. the mother's mother, in childcare, see Iliev 2001.

> My husband understood me. We worked together; we supported each other. Whenever I wrote something, I would always show it to him first. I didn't dare publish a text without having him read it first. Whether it was good or it wasn't. He understood me and helped me – as best he could.'

She, too, had to combine science and family in her everyday life and felt responsible for the processes of negotiation involved in this.

The constant confrontation in everyday life of the roles of mother and scholar and the difficulties in reconciling gainful employment and family had a specific feature in socialist Bulgaria: due to a general lack of work-space at the humanities departments, research work mostly had to be done individually at home. The feminization and perception of certain sciences as 'good professions for women' is in part a result of this fact.[28] To be able to work at home is felt by female scholars as a certain relief, because it makes it easier for them to fulfil their household and childcare duties. At the same time, women in particular declare that they 'naturally' had to take on all the household chores, and their (scholarly) work received little appreciation. Women are confronted with diverging role expectations to a higher degree: they have to fulfil traditional female roles, while at the same time they are expected to study the academic discourse and its modes of behaviour.

This can result in a feeling of insecurity concerning one's roles, expressed in questions such as 'Am I capable of scientific work at all?' or 'How and where do I fit into the academic world?' – particularly by women of the first generation of scholars.[29] It is characteristic that insecurities, interpretation patterns of 'having been lucky' or 'having had the great chance to' come up more frequently in the life histories of women than those of men. The fear of failure, which is much more pronounced in women, on the other hand raises the question of whether this pattern of argumentation ('insecurity', coincidence) might not be linked to 'traditional' gender stereotypes? Socialization has up to the present day been focused on learning gender-differentiated role models and on the development of gender-specific characteristics; surveys show that women are still perceived typically as insecure, dependent and emotional (List 1987: 28). Maybe the (conscious or unconscious) emphasis on a fear of failure on the women's part perpetuates and reproduces the 'ideology of femininity' – insecurity and a need for

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[28] Cf. M1's quote about working outside of one's home 'like all the other people'.

[29] F2: 'At this very moment, I started to have my doubts, in fact I had always had this doubt whether I am suitable for scientific work.' For controversial/incompatible role expectations, with which especially female scientists from lower social strata are confronted, see Sabine Strasser's work, using the example of Austria (Strasser 1998: 27).

protection? As a result of the socialization process and/or as a strategy to succeed in still male dominated structures?

It is characteristic in the life-historical accounts of female Bulgarian scholars that gender-related contradictions are not an explicit issue. This could be accounted for by the higher numbers of women in academic institutions, which concealed rather than made obvious the hierarchical mechanisms and structures of organization; being a woman was not a special status in Bulgarian academic life. The essential lack of gender-sensitive issues is not only the result of immediate directions 'from above', but also of internalizations owing to gender-specific socialization and massive gender-specific role model propaganda.

> X2: It is an objective fact that many start in the same way as I have. Most also experience the same conditions, some even much better ones. Some have a better start. You know that if someone stands behind you and you start out under better conditions – with a nice flat, support from this side and that, on the job and at home – you can accomplish so much more. Still I have achieved much more than everybody I started out with. There are only few who achieved what I have achieved.

The emphasis is not on gender differences, but on the fact that unlike other persons, one succeeded on one's own, without connections or patronage, without 'games'. This accentuation is influenced by the current discourse on socialism as a particularly clientelistic system as well as by the pressure to justify, triggered by this discourse. Female scholars have – as already indicated – incorporated many of the traditional 'typical male' characteristics into their self-conception: intelligence, independence, a critical distance towards authorities of opinion, a readiness to take the initiative, courage and assertiveness.

In any case it is a legitimate question whether following this ideal type might not prove to be a hindrance to discerning the pitfalls of academic life with regard to gender and hierarchy and the (re)production of dependencies (Strasser 1998: 27).

The three life-history interviews that follow were conducted with scholars born in the 1960s: two men and one woman whose careers give insights into postsocialist developments of the discipline.

After the collapse of communism, the humanities especially – which were intended for an ideological role and which could not report any economic usability – remained insufficiently remunerated. At the same time, research is becoming more cost intensive in the humanities, too, due to increased demands. The structural changes intensify the competition for the

distribution of scarce research resources. Possibilities of financial support are mainly based on individual projects and external funds.

This way, the significance of formal and informal networks, of the symbolic and cultural capital, the habitus, is increasing. What increases is not only the importance of horizontal networks, but also of vertical ones (right up to cliques?).

The interviews with scholars of this generation with regard to safety networks (horizontal and vertical ones) on the one hand prove the continuation of certain gender-specific differences: there still exists a kind of male-organized socialization (male discussion groups with M2, partly also with M3). But furthermore, a new increase in the significance of one's social background can be observed, which today is becoming the decisive factor.

The examples portrayed briefly here contrast particularly distinctly: M3 has a cultural capital, 'inherited' from his family (both parents had a successful academic career in the humanities), secured by his education (grammar school with extensive language teaching). This 'cultural competency' makes him feel secure in the scientific arena, his knowledge of the 'rules of the game' gives him access to informal and formal networks and helps him obtain secure and high positions. In his descriptions, the renewed significance of mentoring becomes obvious.

M3 stresses his shift from 'classical folklore to Anglo-Saxon anthropology' thanks to being trained by a Bulgarian anthropologist who returned from working in Canada as a scientist. His brief and longer research and study visits abroad (to England and France) as well as his numerous participations in projects are of major importance for the development of innovative approaches. He talks about such opportunities with a certain lightness: 'You can always go somewhere. For one year, for two years, for five years. There are possibilities …' These possibilities have considerably increased in number in the course of the past two decades and play an immense role in the development and transfer of knowledge. The predominant practice of application and submission requires different and versatile competencies and personal abilities. Even the independent application options via the internet pose a new challenge, i.e. an investment of time and money, for scholars not familiar with them. In addition to the need for references etc., this increases the importance of one's social background, of both horizontal and vertical networks.

I also have this type of social affiliation and, in part, a comparable professional career. Brought up in an 'educated' milieu – my father was a 'travel cadre' (although not in the academic field), my mother an art historian, my grandparents teachers – it was 'natural' for me (and my family) to opt for a higher education: German-speaking grammar school, university

degree, doctorate. In combination with the appropriate habitus and knowledge of unwritten ways of behaviour, this facilitated my uninterrupted scientific career.

In contrast to that, F3 and M2 belong to the first generation of scholars, even though they grew up in families supportive of their university education. Both visited 'normal' instead of so-called 'elite' foreign language schools. The long years of economic change forced them to additionally find sideline – or rather regular – jobs in order to be able to finance their research and/or simply to survive and to support their families. The numerous temporary extra jobs in these cases have little relation to science, which burdens the time and energy budget considerably and has repercussions on their research work and confidence. Feelings of anxiety and of insufficient knowledge are articulated more frequently, even though dynamic features, such as a willingness to change and learn, become fundamental criteria for success, and coping with insecurities is given priority and is defined as an achievement.

Life histories like M3's and mine, with a relatively smooth university or academic career, which simultaneously opens up various different possibilities, proceed comparatively uninterrupted. Such lives today are rather the exception.

In spite of the relatively high number of personnel at academic institutes in Bulgaria with hardly any staff cuts after the collapse of socialism, professional careers are becoming significantly more discontinuous. The young generation of scientists in general is less and less able to produce an 'uninterrupted *curriculum vitae*'. A combining of different jobs simultaneously and in parallel, frequent changes of positions, periods of professional breaks increasingly have become the norm.

The emergence of the 'patchwork jobber' (Russo 1999) promotes personal commitment and versatility and has great potential for innovation, but at the same time it holds the danger that much biographical and cognitive potential for science might be lost.[30]

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[30] For comparable developments in the German-speaking region, see Dressel 2003: 228.

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PART THREE

SERBIA

Chapter 11
Brief Encounters, Dangerous Liaisons and Never-ending Stories: The Politics of Serbian Ethnology and Anthropology in the Interesting Times of Yugoslav Socialism

Slobodan Naumović

> I want to argue, however, that what limited our success among the powers constitutes anthropology's strong point, and distinguishes it from its more prominent sister disciplines.
>
> (Eric Wolf 1999: 132)

Introduction

In what was intended to become the last in a series of 'apologetical retrospections', the late Clifford Geertz took to the task of relating the development of anthropology as a field of study to the 'broader movements of contemporary history'.[1] Sketching his 'broad-stroke' approach to writing about 'the anthropological life' in 'interesting times', Geertz shared with his readers an old wisdom of the trade according to which 'there is very little in anthropology that is genuinely autonomous' (Geertz 2002: 2).[2] In a wider ranging and less-personal, but equally broad-stroke effort to situate 'anthropology among the powers', Eric Wolf, the other towering figure of twentieth-century anthropology who left us recently, proposed to understand 'the history of our endeavour, especially social anthropology, not only as an

[1] As always, Geertz could not but suspend his readers in tightly knit webs of significance, this time spun around Eric Hobsbawm's reflections on how 'interesting times' shaped his own experiences of a 'twentieth-century life' (Hobsbawm 2002).

[2] At a critical moment in the history of the discipline, the late Eric Wolf phrased the same wisdom in a more pronouncedly deterministic fashion: 'Yet in no case could American anthropology escape the dominant issue of the time, and its intellectual responses could not and cannot help but direct themselves to answering it, or to escaping from it. To that extent, at least, the problems of the day enter into how we construct the picture of reality around which we organize our common understandings. As the reality shifts and changes, so our responses to it must shift and change' (Wolf 1974: 253). With age, his determinism seems to have shifted and changed into a mellower and more nuanced outlook.

unfolding of ideas inside the discipline but also as it was shaped within a socio-political environment' (Wolf 1999: 121). While both authors consistently upheld the merits of social and political contextualization of the production of anthropological knowledge, they were equally eager to warn that it does not 'serve us to see all modes of anthropological thought and practice as … linear effects – as 'offspring' of capitalism or as 'handmaidens' of colonialism …' (Wolf 1999: 121). Wolf and Geertz were certainly not the inventors of the genre, but the intelligence and elegance that marked their efforts set new standards for those who follow suit.

In this paper I intend to turn the 'lights', made available by the genre of broad-stroke retrospective analysis, on the politics of Serbian ethnology and anthropology in the 'interesting times' of Yugoslav socialism. Namely, of the more than one hundred years of its institutionalized history, Serbian ethnology functioned under a highly specific local brand of socialism for almost half of the period. During these more than just 'interesting times', ethnology in Serbia was anything but 'genuinely autonomous' and just about as 'inconstant' as Geertz would expect it to be.[3] Therefore, to outline the succession of periods and generations, in society in general and in Serbian ethnology as such, and to trace the interplay between the 'powers' of the Yugoslav brand of socialism and ethnology as a field of study is to make possible the understanding of the discipline as it exists now, both as far as dominant flows, as well as counter currents are concerned.[4] To do this is to move closer to having an idea about what was and what wasn't autonomous in Serbian ethnology and anthropology during socialism, in which way, and why. It is, in other words, to gauge in what ways was Serbian ethnology a 'creature of its time' and a 'relic of the engagements' of its proponents (Geertz 2002: 2).

Geertz and Wolf might have added to the genre an exuberant wizardry with words or a restrained elegance of expression, dazzling erudition, and the equability and wisdom of mature age, but what broad-stroke retrospective analysis presupposes as a must is the profound insider knowledge of the

[3] Joel Halpern and Eugene Hammel were among the first to relate the development of ethnology in socialist Yugoslavia to the shifting political contexts (Halpern and Hammel 1969). Interesting and thought-provoking attempts of political and social contextualization multiply after 1989 (Slavec Gradišnik 2000; Sárkány 2002; Skalník 2002; Rihtman-Auguštin 2004; Hann, Sárkány and Skalník 2005; Kovačević 2006).

[4] Geertz envisioned his project as consisting of two intertwined tasks, the first being to 'outline the succession of phases, periods, eras, generations, or whatever, both generally and in anthropology as such… in the last half of the last century', and the second to 'trace the interplay between (for the most part, American and European) cultural, political, social, and intellectual life overall and anthropology as a special and specialized profession, a trade, a craft, a *métier*' (Geertz 2002: 2).

processes that are being described, which they so aptly demonstrated. In the absence most of these ingredients, this endeavour must take a different, painfully more pedestrian course.

The reasons for doing so are many. First, my personal experience of the interplay of Yugoslav socialism and Serbian ethnology is only partial, mostly covering the eighties, when the system had abandoned most of the 'strong' forms of power it once exercised, and had lost direct leverage over the discipline. Second, my personal memories of socialism as a living political system have scarcely more historical depth, not reaching further then the seventies, to the 'golden years' of Yugoslav socialism when its advantages where most visible and its tragedy inducing potential not yet fully revealed. Third, I was socialized in an anti-communist and anti-Titoist family, and the critical stance towards the regime decisively tainted my memories of socialism as a political project and as a living environment. Fourth, I belong to the cohort of students that were exposed to the first and second generations of ethnological 'modernists' in Serbia, some of whom took great pleasure in strategically downplaying the founding fathers, romanticist heritage and dominant trends of Serbian ethnology. In that sense, for my generation reflection about disciplinary history was from the early student days inseparable from emotionally charged issues of disciplinary politics and from having to take sides in ongoing generational, ideological and theoretical disputes. Finally, I am painfully aware that attempting to compress half a century of science-making 'among the powers' of Yugoslav socialism into a hopefully coherent body of text, while at the same time striving to provide helpful hints for the reading of other texts on Serbia cannot but increase the temptation to simplify and to overstate deterministic and teleological motives.

I have attempted to deal with these very real threats by combining a range of 'antidotes' that were inspiringly applied in some of the fine chapters from the previous volume on socialist era anthropology in East-central Europe (Hann, Sárkány and Skalník 2005), already a work of reference on the interplay of political power and ideology and a marginalized discipline. The following antidotes were applied, albeit to different degrees:

1. creating an analytic chronology of main trends in Serbian ethnology (see the timelines in this volume);
2. supplying a detailed political contextualization of the developments in Serbian ethnology during Yugoslav socialism;
3. theorizing the logic of production and social consumption of ethnology as a discipline (the model of national science- see Naumović 2005, the double-insider syndrome- see Naumović 1998, strategic marginalization of 'unusable' sciences under socialism…);

4. juxtaposing of different generational perspectives;
5. harmonizing insider and outsider views;
6. balancing the need to discern dominant disciplinary trends (such as, for example, the anthropologization of ethnology – see Kovačević 2006) with the idea of disciplinary history as a complex, contradictory and open-ended process (Vincent 1990).

The Politics of Ethnology in Serbia before 1945

In order to understand the ways in which Serbian ethnology was treated after the revolution, one has to outline the challenges that the new regime associated with it. It is, therefore, useful to outline the basic features of pre-revolutionary Serbian ethnology.

First, from its formative period, Serbian ethnology demonstrated a basic similarity with what is now termed 'native anthropology'. In other words, it represented the empirical study of a given society performed by persons who have been fully socialized in it, and saw as their primary task the studying and salvaging of its culture. Because of their primary interest in the 'Own', these professionals lacked the incentives, opportunities, methods and theories needed to exercise the 'view from afar', so characteristic of the anthropological tradition that focuses on the 'Other'.

Second, ethnology in Serbia was decisively marked by the legacy of Vuk Stefanović Karadžić, the great philologist-reformer, predecessor of Serbian folkloristics and ethnology, and the driving force of Serbian Romanticism. Though he never exposed his system of thought coherently, it can roughly be described as incorporating the following Herderian assumptions:

(a) every nation is characterized by a particular *Volksgeist*, and thus it is by the richness of its *Geist* that the contribution of every *Folk* to the treasury of humanity should be measured, and not by the role it had in the making of political history;

(b) the *Geist* is not distributed equally among all of the members of a community – peasantry, the simple *Volk*, is the principal possessor and guardian of the Serbian *Volksgeist*, which is why the sentiment of *Nationalismus* is the strongest and purest among villagers unspoiled by foreign influences;

(c) the language of a *Volk* is the most important expression of its *Geist*, it unites and demarcates the *Volk* more efficiently than any other single cultural trait; therefore, it is owing to their common language that the Serbs of all three 'laws' (that is, religions) can feel themselves, and be considered by others as Serbs, and thus the language of the *Volk* should become the literary standard for all Serbs;

(d) the *Volksgeist* also expresses itself in various forms of popular culture, and particularly in oral folk literature and folk customs, thus they have to be meticulously written down and preserved, for it is on them that the culture of a nation should be founded.

Third, Karadžić's programme was applied, further developed and modified by several generations of amateur and professional researchers. On the way, ethnology, which was unambiguously defined as a theoretical and comparative scientific discipline in the key programmatic texts from the period of its institutionalization (Djordjević 1906; Erdeljanović 1906; for a detailed discussion of their ideas, see Rakić 1997), was remodelled and fully constituted as a *national science*. It reached its highest point with the theoretical and methodological conceptions of the anthropogeographical school founded by Jovan Cvijić, the great geographer, geomorphologist and founding father of Serbian ethnology. Cvijić offered the first comprehensive local scientific 'template' to the discipline. The template incorporated a consistent theoretical grounding, an elaborate research methodology, the regional monograph as a standard for the presentation of research results, a clear conception of the social and political roles of the social sciences, and finally a well organized team of motivated followers (Ćulibrk 1971).

Fourth, anthropogeography and ethnology were included in the prestigious group of principally humanist sciences (philology, history, history of literature, legal history, art history) that were charged with the performance of tasks having 'national' importance, of which the following were most important:

a) confirming that the nation in question really exists (by helping in the enforcement of cultural and linguistic unity upon heterogeneous peasant populations, in other words, by creating it in the modern meaning of the word);
b) confirming that in its pretensions to an independent state the nation had historical legality (by unveiling forgotten glorious kingdoms or inventing them if needed), and/or cultural legitimacy (by gathering massive volumes of indigenous oral literature, by proclaiming that they are of Homeric quality, and by insisting on their uniqueness);
c) proving that the nation had a continuity of territorial possession (ideally by invoking the principle of 'autochthonous descent', and by sketching 'ethnographic charts' that legitimate territorial pretensions of given ethnic groups by outlining their spatial distribution);
d) collecting, borrowing or inventing if needed, and then systematising and integrating the diverse elements that will form the corpus

of a working national culture (standard language, literary canon, values, ideals and role models, customary law, etc.) capable of securing internal cohesion and international prestige;

e) inventing 'liturgies' and creating 'shrines', where the cult of the Nation can be performed under adequate professional scrutiny (the prototypes of which were folklore festivals and national ethnographic museums).

Fifth, out of the reasons that were outlined above, as individuals actively engaged in the public arena on tasks of creating, promoting and advocating their own culture, ethnologists, anthropogeographers and other national scientists tended to be taken in by the double insider syndrome (Naumović 1998). The syndrome consists of three mutually interconnected segments:

a) because of being born and/or socialized in the studied society or culture, and because of more or less consciously identifying with its language, traditions, historical memories, dominant values and real or supposed interests, the ethnologist tends to develop a profound emotional attachment and moral responsibility to the object of his study, which under certain circumstances brings about the loss of his impartiality;

b) because of considering himself, and of being considered by others, to belong to the special social subgroup (publicly and/or politically engaged intellectuals), whose task is to study, consolidate, and, if needed, invent the identity and interests of his society/culture, as well as to defend them by force of arguments, the ethnologist assumes the role of the advocate both of his object of study and of his native society/culture;

c) because of interiorized responsibilities, as member and advocate of his society or culture, the ethnologist is obliged to react if he perceives it to be in a position of political or any other kind of inferiority, and particularly when his society and its culture are publicly perceived as being threatened or victimized.

Sixth, because the field was within easy reach, and because research could be done in one's native tongue, there was no need for fieldwork in the anthropological sense of the word. Filling out questionnaires, doing research in historical archives, accumulating data on the long run, and occasionally registering changes in the field made more sense than engaging in long-term participant observation, or so it was believed at the time of institutionalization of the discipline.

Seventh, the consequences of redefining ethnology as a national science practised by double insiders can perhaps best be described with the list of four '-isms'. The first, *localism*, resulted from a double restriction that the

project of national science enforced on Serbian ethnologists. They were to study only their own group, and even then, not all of its members. Because they were believed to possess, express, and guard the essence of nationhood, only the *Volk* or simple peasants were considered to be fully legitimate objects of scientific interest. The second -ism, *empiricism*, stemmed from the 'urgent need' to 'collect' and 'preserve for eternity' all the rapidly disappearing emanations of *Volksgeist*, in the hope that the true essence of the Nation would thus be saved for posterity. The next -ism, *anti-comparativism*, functioned as a methodological device for the preservation of myths of uniqueness of the Nation. Lastly, if anti-comparativism protected the mystified characteristics of the object of study of national sciences, *anti-theorism* protected the whole edifice of national sciences, or more precisely, the basic principles upon which they were built, from critical scrutiny.

Finally, what preserved ethnologists as practitioners of a specific version of national science from becoming nationalist propagandists was (a) the shared conviction that the only acceptable and effective way of advocating the cause of one's group was by the force of scientific arguments (various versions of positivism), as well as (b) the belief that the national mission of the intelligentsia should be inseparable from unsentimental criticism of intellectual, cultural and moral flaws of one's community (Cvijić 1921a: 3–49, 51–71).

Therefore, the simplest answer to the question concerning the character of the science that the new communist regime had to face is: it had to deal with a fully institutionalized and very prestigious national, or nation-building science, whose practitioners were in most cases affected by the double-insider syndrome, and were functioning as the *avant-guarde* of the Serbian, or eventually of the Yugoslav nation (Pavković 1995). As such, they could be seen as coming very close to being direct rivals for the social and political roles that the communists saw fit for themselves. However, the romanticist twist that was keeping ethnology on the tracks of age-old peasant traditions made the discipline appear somewhat marginal to the future-bound doctrinaires of communist utopia. Thus, they had before them a potentially very dangerous ideological and political foe, but one that was less threatening than other strategically better equipped adversaries, say, 'bourgeois' historians, legal scholars or anti-dialectic, 'abstract' philosophers.

On the other hand, Serbian pre-revolutionary ethnology managed to produce only a very small scientific community. Furthermore, at the time of contact the discipline was undergoing a drastic replacement of generations. Tihomir Djordjević and Jovan Erdeljanović, the leading scholars of the discipline, both died in 1944. With Jovan Cvijić gone in 1927, Sima Trojanović in 1935, and when Veselin Čajkanović, the classicist and leading

authority on the history of Serbian religion, departed in 1946, the discipline was left without the most prominent members of the generations that made it into one of the leading national sciences. Because of those and other reasons, by the thirties the discipline had passed its high point in terms of theory production and the capacity for knowledge transfer. Ethnology might very well have looked a formidable ideological enemy, but it was also quite clear that it was well past its prime.

Serbian Ethnology in the Interesting Times of Yugoslav Socialism

In Yugoslavia the communists came to power after winning both of the wars that they simultaneously waged from 1941 to 1945. The liberation war (narodnooslobodilačka borba or NOB) fought principally against German, but also against Italian, Hungarian, Bulgarian and Albanian occupying forces attracted more international attention as it could divert some of the Axis forces from other battlegrounds and thus reduce allied losses. Even though they were substantially helped by Western allies (UK and US in particular), who after 1943 supplied arms and logistical support to Tito's fighters, and by the Red Army, which directly participated in some of the decisive operations during 1944 and 1945 (notably the liberation of Belgrade), the communists could rather convincingly boast of winning the war mostly by themselves. Thus, victory against 'fascist intruders' in 'the battle for people's liberation (NOB)' became one of the key legitimating formulas of their rule.

However, at least as important, if not more so, was the civil war that communists waged against a number of local political opponents. These nationalist and anticommunist forces principally defended the interests of their mutually confronted nations and/or religions.[5] For the communists, the crucial importance of their victory in the civil war resided in the fact that it presented them as the only force capable of stopping and perhaps rooting out national and religious hatred, the plague that threatened the very existence of Yugoslavia since its inception. 'Brotherhood and Unity of our peoples forged in the liberation war and dear to us as the apple of our eye' thus became the second, and perhaps most powerful formula of legitimacy of the

[5] The Serbian monarchist loyalists (the Chetniks, lead by General Draža Mihailović) were regarded by the communists as their main rivals because they remained the only legitimate representatives of the exiled Yugoslav government and because they combined resolute anticommunism with Serbian nationalism. Other bitter opponents included Croatian fascists (the Ustashas, headed by Dr. Ante Pavelić) – who set up a puppet state backed by Nazi Germany ('The Independent State of Croatia', NDH, which zealously strove towards the 'final solution' of the Serbian, Gypsy and Jewish 'questions' in the state – as well as Serbian counterparts such as the collaborator General Milan Nedić, and the Maurrasian-type fascist Dimitrije Ljotić, founder of the political movement Zbor.

new communist regime. Due to the inherent instability of the Yugoslav state, it also came to be the longest lasting one. Issues of ethnicity, national relations, nationalism and federalism thus became central political, but also theoretical problems in the post-war period.

Apart from being labelled as the people's liberation war and the period of the forging of brotherhood and unity, the struggle for power from 1941 to 1945 was also ideologically interpreted as incarnating the Yugoslav revolution. However, Tito decided to proceed with the introduction of radical political and economic changes (large scale nationalization and expropriation of private property, agrarian reform and collectivization of land and so on) only when, after winning fake elections against a beheaded democratic opposition, he became confident that the country was firmly in his hands (Koštunica, Čavoški 1983). Paradoxically, the Yugoslav revolution had to borrow its legitimacy from the charisma of the idealized war years and to hide its true goals under a plethora of rhetorical formulae. In that sense, the revolutionary discourse represented the weakest of the three pillars of legitimacy of the Yugoslav communist regime, even in its early phase.

The new regime had to face other problems of vital importance to its survival apart from issues of legitimacy. It had to find ways to rebuild the infrastructure of a country devastated by war, but also to change the ownership structure and functioning of its pre-war economy. The rapid industrialization of what used to be a predominantly peasant country was vital for economic reasons – the need to create a sufficient basis for the country's more or less independent existence – but for ideological reasons as well. The peasantry as the most numerous social class had to be rapidly reduced in order that an adequate social basis for the prophesied 'socialist state as the state of the working class' be created. The fulfilment of an even more ambitious prophecy also had to be attempted. Namely, it was proclaimed that socialism was to catch up with capitalism within a decade or two, and then surpass it in all standards. Furthermore, Tito's regime, which already started to demonstrate an unusual amount of independent-mindedness, had to find a way of defining its position in the socialist family, in which the USSR saw itself in the role of the indisputable 'elder brother'. Finally, and perhaps most importantly, the communists had to offer proof of their capacity to handle an extremely fragile multi-national and multi-confessional country not only in times of war, when their only alternative was the raging ethnic and religious conflict, but in times of peace and relative prosperity as well.

In order not to be crushed by such complex problems, but also out of the need to accommodate itself to the shifting international political contexts, the communist regime constantly had to envisage changes (baptized as 'reforms') in the economical and political spheres (Sekelj 1990). The strate-

gic moments in which such changes were introduced form the ground for various possible periodizations of the era of socialism in Yugoslavia. Presented here will be a very rudimentary tripartite scheme based principally on key political reforms. The period of socialism started with a short *orthodox phase*, during which the replication of the Soviet model was attempted (1945–8 and even until the first half of the fifties/Stalin's death). Next came the *workers' self-management and non-alignment phase*, lasting for little less than three decades. During that phase the regime boasted both of attempting to, and of succeeding in finding an 'original road to socialism', as well as of providing economic prosperity for its subjects. Taken together, these refinements earned for Tito's regime the flattering label of 'socialism with a human face'. The self-management and non-alignment phase itself can be divided into the federalist and the quasi-confederalist phase. The latter was anticipated by the demands for national independence made during the Slovenian 'Road affair' and more resolutely by the Croat ultra-nationalist 'Mass movement' in 1971. As in other instances of profound crisis, Tito's strategy was first to eliminate the leading figures of revolt, and then to satisfy their more salient demands, albeit presenting them as his own refinements of the system. As a result, several rounds of constitutional amendments were introduced, followed by the new constitution of 1974. Yugoslav republics and autonomous provinces thus gained sovereignty comparable to that of independent states, and the process of disintegration of Yugoslavia took an irrevocable course. Finally, the decade following Tito's death (1980–90/1991), which can be labelled *the phase of aggravating economic and political crisis*, prepared the stage for the break-up of Yugoslavia.

1945–9: Together with Comrade Stalin, for Better or for Worse

The strategies developed by the communist regime in order to resolve internal problems and to adapt to changes in international relations profoundly shaped the fate of ethnology in Yugoslavia and Serbia.

As far as developments in ethnology are concerned, the consequences of *ideological orthodoxy* characterizing the *first phase* of the socialist period in Yugoslavia and Serbia manifested itself in several distinguishable but interconnected ways.

The rudest form of influence had to do with the brutal demonstration of force by the new regime. To take but one example, in April 1945 the communist controlled Court of Honour of the University of Belgrade expelled from the University Veselin Čajkanović, professor of Belgrade University and member of the Serbian Royal Academy, prominent classicist, philologist, and leading authority on the history of Serbian religion, on false

accusations of collaboration with the quisling regime of general Milan Nedić. Čajkanović was publicly humiliated, left without resources, and his personal library was confiscated. After receiving such blows, the disgraced elderly academician fell ill and died in less than a year (Aleksić 1998).

The second form of influence had to do with the demonstration of the regime's creative powers, and with the desire to subjugate and permanently control former leading national institutions. Thus, in 1947 a new research Institute was created in the frame of the completely reorganized Serbian Academy of Sciences – the Ethnographic Institute (Etnografski institut SAN, more on the institute in Prelić, in this volume). The name of the new institute seems to point to Soviet influences. Namely, following the critique of ethnology as a reactionary bourgeois science, the name of the discipline was exorcized in the USSR and replaced by the ideologically correct one – ethnography (Gorunović 1997: 376). However, the new name could also be seen as respecting the tradition set forth by the Ethnographic Board (Etnografski odbor) of the pre-war Serbian Royal Academy. While the dilemma might open up the issue of the regime's readiness for pragmatic compromises with politically usable pasts, the very idea of an academy of sciences functioning as the central state-controlled research institution, recognizable in the organizational structure and scientific aims of the new institute, clearly resembles the Soviet model. The new institute was expected to 'organize systematic and planned research on the settlements and origin of the populations, folk life, customs and beliefs, as well as folk tradition in our country and among all our peoples (Pantelić 1997: 23).' It was also meant to represent the highest ranking research institution of the discipline of ethnology. Interestingly enough, the name of the Department of Ethnology at the Faculty of Philosophy in Belgrade was left unchanged. Even more interesting is the fact that Cvijić's anthropogeography as a scientific template found ample place in the new scientific paradigms, even though Cvijić's research on psychic types came under fervent ideological criticism as the ideological basis of 'Greater Serbian hegemony'. Thus, while there is indubitable evidence of Soviet influence on organizational issues and scientific policies in this early period, the accommodations seem to have been administered in a piecemeal fashion, without cutting all connections with the institutions and practitioners of the vanquished 'bourgeois science'.

The next type of political influence on ethnology during the phase of ideological orthodoxy can be associated with the strategic decision of the regime to intensify the growth of folkloristics as the novel discipline that could become the functional equivalent of the former national sciences (Naumović 2005), albeit in the new anti-nationalist socialist political setting. The initial fondness towards folkloristics certainly had much to do with the

understanding of Vuk Karadžić as a 'complete revolutionary who strikes down the old and creates the new' (quote after Nedeljković 1975: 12). Of equal importance was the fact that the discipline was not formally institutionalized as a national science in the pre-revolutionary period. Finally, because of the considerable degree of state control exercised formally and informally over it, the new discipline was expected to handle the potentially explosive issues like the study of ideologically important forms of folklore, or the public displaying of symbolic manifestations of national identity at folklore festivals in a way that could not have been expected from ethnology (for an insider's view of the development of the discipline, see Antonijević 1983). Interestingly enough, the new discipline was not accorded a fully independent organizational frame, but was in the early years kept in the frame of a section of the Ethnographic Institute. Some of the scientific activities in the frame of the new discipline of folkloristics, as well as certain individual and group practices of those associated with it offer further evidence of the ways in which science actually functioned during the period of ideological orthodoxy. Namely, the Marxist philosopher and ethnologist Dušan Nedeljković, incidentally a member of the Court of Honour of the University of Belgrade which expelled Veselin Čajkanović from the University, commenced in 1949 a long term collective research project in the frame of the newly formed Folklore Section of the Ethnographic Institute of the SAS on the collection and analysis of Yugoslav partisan folklore. The project assembled a large research team that collected more than 20,000 partisan songs. Several collective and individual monographs came out as a result, as well as numerous articles. Among the outputs, on could note Nedeljković's paper on 'Comrade Tito in popular songs' (Drug Tito u narodnoj pesmi), in which he demonstrates by applying the 'concrete dialectical methodological approach' that Tito was the 'symbol of monolithic fraternity and unity of our peoples' (Janković 1969: 7).

Nedeljković's ostentatious 'concrete dialectical socio-historical Marxist methodologically grounded approach' (for details, see his dialogue with Antonijević 1969; for a devastating critique of Nedeljković's 'approach' by a fellow Marxist, see Kulišić 1967), in the frame of which he somewhat awkwardly combined the ideas acquired during the years spent at the Sorbonne (1917–19) with a self-styled 'revolutionary' mixture of Soviet 'dialectical Marxism' and Serbian romanticist populism, together with the personal influence that he managed to exercise over a number of his colleagues, can both be seen as paradigmatic of the ways in which politics exercised a hold over science in the period of ideological orthodoxy. Namely, some of the researchers engaged in the project on partisan folklore with time became a corporate group over which Nedeljković exercised control and patronage not

only as its scientific leader, but also in the style of a powerful *éminence grise*, masterfully pulling the strings of other people's careers even after he fell out of political grace in 1953. Nedeljković had the habit of offering ideologically correct 'theoretical and methodological guidance' to members of his younger *entourage*, at times with bizarre consequences. Thus he suggested that his young protégé Mirko Barjaktarović should put forward in his thesis on rural landmarks (Zemljišne medje u Srba) the idea according to which the origins of the practice of marking plots of land should be sought in totemic beliefs and practices. Several years later, Barjaktarović found himself in the awkward position of having to explain the principles of Serbian peasant totemism to a bewildered Polish colleague (interview with Prof. Nikola Pavković, 1997). From the folkloristic-ethnological networks that Nedeljković created emerged several prominent academic figures, some of whom exercised comparable forms of overt ideological and covert personal influence late into the eighties. Thus, in a paper that attempted to redefine the standards of the new applied 'socialist' social science (Antonijević 1968), Nedeljković's closest protégé Dragoslav Antonijević proposed that the central 'combat task' of Serbian folkloristics should be the fight against 'folklorism'. The struggle against the 'dangerous malaise' should be fought by mercilessly castigating 'folkloristic kitsch', by 'accentuating the authenticity, originality, and purity' of 'autochthonous' folklore, and by putting folklore festivals 'firmly under the control of experts' personified by the Association of Folklorists of Serbia with the author himself as its head. Ideological crusades waged against both internal degradation (symbolized by concepts of kitsch and mass culture) and the equally dreaded penetration by alien cultural elements (usually presented as 'Orientalization') remained the central themes of the politics of Serbian folkloristics well into the eighties.

The next, particularly striking form of influence that ideological orthodoxy exercised over ethnology manifested itself in merciless attacks launched by zealous party activists against the 'centralist', 'unitarist', 'hegemonic', 'nationalistic', 'great-Serbian', and 'bourgeois' spirit that supposedly characterized the discipline in the pre-war period, in tune with the official communist interpretation of the 'rotten' character of inter-ethnic and class relations in the Kingdom of Yugoslavia (Kulišić 1953, more on the issue in Gorunović in this volume). While such attacks obviously aimed at the ethnological double-insider syndrome, their intention at the time was not to demolish it, but rather to replace the national loyalties that it upheld with party and class loyalties. Directly related to the previous form of political pressure on the discipline were ideological attacks directed at Western anthropological theories and at their 'local propagators'. Thus, in one of the more important texts from the period, the functionalist approach was accused

of being 'anti-historical, retrograde, and reactionary in orientation' (Kulišić 1955). As if such an anathema seemed insufficient to the fervent critic, functionalism was further presented as a 'lackey of colonialism' and as a foxy 'manifestation of racism', in a consistent local variant of Patyehin-styled tradition of ideological thinking. Interestingly enough, Kulišić did not forget to mention the 'increasingly evident Panslavic chauvinist tendencies' that characterized 'Soviet absolutism and the absolutism of Russian science', thus providing lip service to Tito's novel renegade political course (Kulišić 1953). Quite understandably, proponents of the alien malaise had to be found among our own ranks as well. Thus, Kulišić attacked Milenko Filipović, one of the most prominent Serbian ethnologists of the period, for importing the functionalist 'anti-historicist' plague into Yugoslav ethnography (Kulišić 1955). Filipović's reply was itself indicative of the tumultuous times, for he stated that he had 'not read any of the more significant works of Radcliffe-Brown, and this is also the case with many of the works of other functionalists' (Filipović 1956, English translation of the passage in Halpern 1970: 19). Neither of the two explanations that Filipović offered for such a state can be considered as convincing. Namely, he argued that (a) as he was interested principally in Serbian ethnology he had neither the interest nor the time to keep himself informed on the whole of ethnological literature; and (b) that it was not possible for him to obtain functionalist works (*ibid*.). However, in 1951 Filipović received a grant from the Rockefeller Foundation for a year-long study in the USA. He spent most of his time at Harvard, working with A. Lord, M. Perry and other scholars. During his stay, Filipović also had the opportunity to meet Joel Halpern in New York, and it was then and there that they developed a lasting professional and personal relationship. Therefore, it seems highly improbable that Filipović could be uninformed about the work of leading functionalist authors, all the more so as his own research was often described as offering some of the first examples of proto-functionalist explanations in Serbian ethnology. It is therefore possible to speculate that when attacked, Filipović chose to downplay his knowledge of an ideologically incorrect field of interest in order to defuse the pressure he was feeling (more on the heated exchanges between Kulišić and Filipović in Gorunović, this volume).

In tune with described ideological onslaughts were attempts to develop a 'truly dialectical, historical-materialist' discipline, which would transcend the limitations both of the 'backward, reactionary, metaphysical, and mechanistic Western anthropology' and of 'Soviet scientific absolutism'. However, such attempts themselves resulted in little more than ritual invocations of formulas from the basic Morgan-Engels repertoire, albeit mostly from Italian and Russian translations of their works, followed by

their somewhat mechanical application to the Balkan context (for examples of such work, see Kulišić 1958, 1963; more on the topic in Gorunović, this volume).

However, perhaps the most effective form of pressure came as a result of politically induced reshuffling between the disciplines that used to be classified as national sciences in the pre-revolutionary period. Namely, because it could integrate with more efficiency the conjectural history at the heart of dialectical materialism, historiography stood out at that crucial moment as socialism's flagship engaged science. On the other hand, because it was burdened by its obsession with a-historic peasant exoticism, and because of its pre-war national-hegemonic sins, ethnology had to be deposed from the prestigious pedestal it once occupied. As a result of this aided fall from grace, ethnology was subjected to informal and semi-formal tutorship by Marxist historiographers, and somewhat later by Marxist philosophers and sociologists. Thus was established a bizarre pecking order between the disciplines, in the frame of which the lowest position was accorded to ethnology. The political elites transmitted their directions to the historians, who retransmitted them further down the line, at the end of which stood the ethnologists, who happened to have no one below them to peck.[6] In his study on post-war Yugoslav historiography, the historian Wayne Vuchinich describes in detail the directives that historians received from the highest echelons of politics, and according to which they had to rearrange their discipline (Vuchinich 1951). His descriptions are of particular interest because parts of the received ideological wisdom were retransmitted to ethnologists in the frame of informal or semi-formal tutorships, or, eventually, in ideologized polemics, like the one on the origin of Montenegrin tribes (more on the polemic in Gorunović, this volume). Vuchinich thus states:

> Above all else, the historian is expected to adhere closely to the Marx-Lenin teachings and philosophy of history. He must not deviate from the 'ideology of the international working class' and 'historical materialism', which represent the only 'scientific' historical method. The Yugoslav historian must realize that a long and arduous road lies ahead, that his country is now in a transition stage from capitalism to socialism, and that there can be no compromise between proletarian and bourgeois ideologies (Vuchinich 1951: 41).

After presenting the general ideological frame, Vuchinich goes on to list specific tasks that lie before the historian in the 'transition stage':

[6] The most devastating consequence of this academic re-shuffling was that ethnology lost the legitimacy to perform certain 'strategic' tasks, like the systematization of nationalities, as well as to offer expert advice on issues related to federalism and nationalism.

> The Fifth Congress of the Yugoslav Communist Party approved the basic directives for the work of Yugoslav scholars. They are first directed to strengthen Marx-Lenin propaganda within the party and among the workers and to emphasize the study of historical materialism. All 'revisionism' must be eliminated, and 'idealism' and 'mysticism' removed from schoolbooks. The history of individual Yugoslav regions, the Yugoslav Communist Party, workers' movements, the NLM, and developments in postwar Yugoslavia must be placed under systematic Marxian scrutiny. The new Yugoslav historian must reinterpret the written history of his people and fill the gap in existing historiography.
>
> The first and fundamental task of the new historian […] is a revision and critical evaluation of the old legal historiography. Idealistic, reactionary, and unscientific theories must be rejected, and the 'objectivism' of the bourgeois historians exposed and condemned. Among theories singled out for refutation are those which emphasize the superclass status of the medieval states, the extraordinary state-building abilities of the Serbian and/or Croatian people, and the origin of the state through conquest.
>
> The second important duty of the new historian is to investigate the history of those peoples whose past has been neglected or insufficiently explored. In prewar investigations the emphasis was on the field of Serbian and Croatian legal and institutional history; little or nothing was done with Slovene, Bosnian, Macedonian, and Montenegrin history. Similarly, certain epochs – the Nemanjić period in Serbian history and the period of native rulers in Croatia – received far greater attention than others. According to Djilas, the revolutionary periods in Yugoslav history have been neglected, completely ignored, or misinterpreted, and crucial periods, such as the first Serbian revolution, have been colored with monarchist and national prejudices [. . .].
>
> The third major duty of the new historian is to study the history of modes of production, the socio-economic development of individual states, the role of the working class, the class struggle, and the conditions under which individual states came into existence. And here, naturally, historical materialism is considered the only valid tool aiding the historian to solve all basic issues! Once these prescriptions are followed, the modern historian, unlike his predecessors, is expected to produce organized, planned, and systematic studies (Vuchinich 1951: 42–3).

Many of the listed ideological 'directives' managed to penetrate actual scientific debates and polemics. Thus, in a series of texts, books and polemical exchanges spanning from the first half of the fifties well into the eighties, the Marxist historian Branislav Djurdjev offered what seemed to be a resolute, but still very well argued and thoroughly documented criticism of Jovan Erdeljanović's 'theory of continuity' and other ethnological explanations of the problem of formation of Montenegrin tribes (for details, see Gorunović in this volume). However, what could have appeared from the outside as a typical tug of war between competing scientific arguments also incorporated a resolutely ideological dimension. Namely, Djurdjev's seemingly convincing solution could also be read as supplying the 'ideologically correct' answer to a scientific problem with fundamental repercussions for the question of the status of Montenegrin nation, based on a doctrinaire Morgan-Engelsian reading of the role of gens in Vallachian *katuns* (pastoralist settlements and kin groups) and very much in tune with the ideological instructions supplied by the party.

While the period of ideological orthodoxy offered some of the most ferocious examples of disciplining ethnology and its practitioners, what sticks out is that by the end of the day it became obvious that the effects of strong power (including the cases of legal and political repression and outright ideological assault) were by far outstripped by what could be accomplished by softer forms of power, in particular by repositioning and marginalizing disciplines, positively engaging the motivation of chosen leading practitioners, and by personalized forms of persuasion in the frame of informal academic networks.

1950–74: On the Road to Socialism, the Yugoslav Way

Let us now take a look at the *second phase*, that of *workers' self-management and non-alignment*, or of the quest for socialism with a human face. The Resolution of the Informbureau in 1948, followed by the failure of Tito's reconciliatory manoeuvres, radically changed Yugoslavia's political course. Stalin ceased being 'our most beloved comrade', and thousands of his admirers who could not, or would not reorient themselves quickly enough ended up in concentration camps of which the most notorious was the one on the island Goli Otok in the Adriatic.

The historian Dušan Bataković describes the dynamic and the consequences of the clash that decided the fate of Yugoslavia for years to come in the following way:

> The rupture with the Soviet Union in July 1948, which directly endangered his authority, was something Tito, as a pragmatic and very adaptable statesman, turned into his greatest success. The famous

> schism intimated that Yugoslavia would take its own road, setting aside the experiences of the Moscow regime. Thus, during the Cold War, Tito won the undivided sympathy of the West which was backed up by considerable military and financial support. The Communist Party of Yugoslavia changed its name into the Yugoslav League of Communists (1952), and the system of self-management (1950) was inaugurated as new doctrine of the internal order presenting an ideological challenge to the Soviet-type real-socialism. Although it was an impossible mixture of empty tirades that created an enormous bureaucratic apparatus and blocked economic development, it was for decades that self-management kept thrilling left-wing western intellectuals as an important innovation in socialism (Bataković 1995).

Thus was born 'the Yugoslav road to socialism' (Sekelj 1990: 9). New formulas were sought, and issues of legitimacy once again came to the fore. Among the more important solutions developed on the road one could include experiments with the restricted reintroduction of market laws into the socialist economy, various forms of workers' self-management, carefully controlled dismantling of the state, the politics of nonalignment, or the system of (semi-)open borders. However, the reforms (both economic and political) never transcended the amplitudes of the 'pendulum model' – they were abruptly stopped whenever they seemed to endanger the central authority of the party or of Tito himself. As time went on, initial legitimization formulas underwent changes. In a period characterized by relative stability, moderately rising standards of living, and partial democratization of political life the revolutionary rhetoric of class struggle and the rhetoric of the liberation war lost their charm. They were gradually replaced by the increasingly esoteric rhetoric of 'workers' self-management' *cum* 'withering away of the state', the international prestige of being a founder of the non-aligned movement and, of equal if not even greater importance, by the symbolism of economic prosperity and by Tito's institutionalized charisma. However, as the tensions between Yugoslav nations and religions failed to wither away as was expected, the rhetoric glorifying 'the most consequent solution of the national question' managed to preserve its privileged position. What changed were not so much the rhetoric formulas dealing with national relations, but rather the envisaged solutions to these problems.

These developments had several consequences for the politics of ethnology.

Ideological orthodoxy loosened its grip, though it continued to reappear in sporadic outbursts. On the other hand, the time was not ripe yet for the massive theoretical influx from the West, which was to characterize

ethnology in the next decade. As could be expected, the pressure on the double-insider syndrome did not cease, it only changed its form from ideological barrage fire to more subtle forms of control. Because of such subtle and often self-imposed restrictions, ethnologists were getting used to studying the 'Own' as if they were 'Others'. The very idea of the Own in the national sense became blurred by the concept of Us, both in the sense of a diffuse working class and, more importantly, in the sense of 'brotherhood-and-unity of the Yugoslav peoples'. Oddly enough, actual field research on 'Others' from other Yugoslav republics, did not gain momentum (Halpern 1970: 3). The result was rather peculiar. Quite timidly, after the razing inflicted by the war and the short orthodoxy phase ethnology started to bloom once again. This time, however, the flowers seemed to have much less ideological or theoretical colour and smell. The autochthonous intellectual traditions linked to the ideal of national science, Cvijić's anthropogeography in particular, were being slowly deserted (at least their political aspect, if not their methodology and the presentation format of regional monography) without new paradigms to replace them.

A learned, if somewhat euphemistic expression for such a phenomenon in one of the later key rethinking efforts of the discipline was that Serbian ethnology was going through 'the phase of empiricism'. Professors Pavković, Bandić, and Kovačević paint the following 'naturalistic' portrait of the sad fate of the discipline in the phase of 'empiricism' (for a similar assessment, see Halpern 1970: 7 and 19):

> Less expressed as a theoretical principle but more as a practice, empiricism has been the prevailing form of ethnological work until the 1960s, although there were some studies of scientific value then as well. Essentially, it meant that theory and empiricism were not sufficiently linked, even that they were implicitly confronted. The attitude to theory was that it is more or less relying on foreign thought that 'has nothing to do' with 'our cultures', our ethnological reality. Collecting materials has been mentioned as the most important task quite a number of times, and if possible, older and 'more original', 'authentic', 'folk' materials should be sought for. Nothing that was not a village culture product was worth ethnologists' attention […].
> But for all this, the collection of materials was not followed by adequate scientific processing. Moreover, modest presence of strategic scientific aims in collecting ethnological materials or even their absence sometimes make these materials lose a great deal of their value for analysis (Pavković *et. al*. 1988: 6–7).

At its best, the empiricist orientation resulted in thorough and reliable work, either in the form of individual texts or of regional monographic studies (see

for example Knežević and Jovanović 1958).[7] The work done by Milenko Filipović can be seen as representing the full potential of such an approach. Thus in 1965 Filipović published his groundbreaking study on the social networks and webs of reciprocity related to the institution of *slava* (feast dedicated to the patron saint of a family) in Serbia (Zvanica ili uzov...). Because of his consistent problem-oriented, dynamic and comparativist approach, very solid ethnography, focus on ongoing social processes, keen analysis of various forms of overlapping of economical, social and ritual contexts, and occasional proto-functionalist interpretations, Filipović can perhaps be considered as the predecessor of what later came to be known as the trend of anthropologization of ethnology in Serbia (see Prošić-Dvornić in this volume).

At its worst, the empiricist approach did end up with thick volumes of regional ethnographic descriptions which, however, lacked any 'thickness' in the Geertzian/Ryleian meaning of the word. In between those extremes, solid research work was being done, in particular in the frame of research projects of the Ethnographic Institute and of the Ethnographic Museum. The Museum was also actively restructuring its organizational frames and introducing interesting innovations in the ways in which it was reaching out to the public. Great advancements were also made in the presentation of Yugoslav and Serbian popular culture, arts and crafts in the international arena (see Cvetković in this volme).

However, while the espousing of the somewhat particular brand of 'empiricism' as a reaction to the pressures of ideological orthodoxy undoubtedly had negative consequences for the development of ethnological theory in Serbia, it had positive effects as far as certain aspects of the politics of the discipline are concerned. Thus, the dismantling of the double-insider syndrome advanced even further during this phase. Not only was ethnology disassociated from the national mission it once espoused, in a paradoxical twist the very idea of engaged ideological approach to the subject of study, in other words of advocacy, came under question. Rather unintentionally, the ground was being prepared for the outbursts of theory that would happen in less then a decade.

Finally, it was at the beginning of this period that the first Western anthropologists got the opportunity to do research in Serbia. During 1953–4 Joel and Barbara Halpern began their fieldwork in the Serbian village Orašac

[7] For an inspiring and humorous comparison of 'native ethnography and ethnography of the other in a visit to the Serbian village', culminating in what the author termed as the 'Šumadija derby' between Serbian ethnologists S. Knežević and M. Jovanović writing on the village of Jarmenovci and J. Halpern on the village of Orašac, see Kovačević 2006: 97–132.

(for a detailed examination of this interesting case and its implications, see Halpern and Prošić-Dvornić in this volume).

1974–80: The Yugoslav Experiment Reaches its Internal Limits

During the early seventies the political developments became even more complex. I will concentrate only on issues that had direct consequences for ethnology. The first had to do with the continuing warming-up of political relations of Tito's regime with the West, resulting in the further liberalization (albeit firmly controlled from the political centre) of certain segments of political, public and economical spheres of life in Yugoslavia. Second, such 'Westernizing' tendencies helped bring about a more pragmatist political ethos, which resulted in the strong favouring of a number of scientific and humanistic disciplines the practice of which, it was believed, could help solve social and economical problems. The third was related to the paradoxical outcomes of the ways in which the national question was treated.

Ethnology, together with other social sciences, humanities and arts could only benefit from further liberalization of Yugoslav society. For ethnology in particular there were two important consequences. Contacts with foreign colleagues, established both while the latter were doing fieldwork in Yugoslavia, or during increasingly frequent study trips of Yugoslav scholars abroad, became even more numerous. This meant that opportunities for the exchange of ideas were increased considerably. Furthermore, foreign books and reviews were imported or translated in quantities absolutely unimaginable only a decade ago. Already during the sixties rare pioneering theoretical and survey studies appeared in which the relations between ethnography, ethnology and anthropology, or anthropology and sociology were discussed in an engaged and polemical style. Thus, Manojlo Gluščević, Filipović's talented student, sharply criticized the conception of ethnology as a science specializing in the study of ethnos and ethnogenesis. The ethnological project was seen as based on 'hypothetical reconstructions' and 'putative history', and anthropology was presented as the way out of this scientific conundrum (Gluščević 1963, 1964). Špiro Kulišić and Mirko Barjaktarović, who as it seems correctly understood that Gluščević's attack was aiming, among others, at their own work, offered a ferocious rebuttal of Gluščević's criticism in particular and his anthropological project in general, trying to defend the conception of ethnology as a discipline that studies the emergence and development of ethnic communities (see Gorunović in this volume).

Later on, more ambitious theoretical treatises were also published, but as a rule by individuals marginal to the mainstream of the marginalized

discipline (either as far as their intellectual 'descent' or academic position are concerned, or both).

In what represents one of the rare projects of anthropological grand theory in Serbia, Zagorka Golubović attempted to develop an integral 'humanistic approach in anthropology' (Golubović 1967; esp. Golubović 1973; for an in depth analysis, see Spasić in this volume). This 'systematic conception' of a general science of man was principally based on Marx's early manuscripts and his concepts of *praxis* and *human generic being*, further developed by ideas of the Frankfurt School and Erich Fromm, as well as on American culture and personality theories. Golubović's approach shared these theoretical influences with the Praxis school of philosophy, the intellectual *enfant terrible* of Yugoslav Marxism, which paradoxically enough became the principal source of its international fame.

There were also less positive consequences for the discipline. Due to the pragmatist trend, labelled 'technocratic' after it fell out of favour, the regime became more interested in the application of scientific knowledge in a number of key sectors like economy or social welfare, but also for the prediction of social and political trends. Thus, sociology, economy and philosophy gained new esteem as disciplines whose findings could be successfully applied, while ethnology had to face further loss of prestige (Halpern and Hammel 1969 situate the beginnings of this trend even earlier, in the late 1950s).

However, as far as the consequences for the politics of ethnology were concerned, the central problem of the period was related to the perhaps unintended but certainly paradoxical outcomes of a number of contradictory political measures devised for the solution of the national question in Yugoslavia (Burg 1966; Rusinow 1988; Ramet 1992; Vujačić 1995). Namely, the basic riddle to be answered was how to make national communities profoundly differing in culture, religion, economic potential, historical experiences, sense of identity and size of population, bitterly opposed during recent wars, and finally, having diametrically opposed interests in and expectations from the joint Yugoslav state, content with their respective positions in the state. Furthermore, the degree of satisfaction had to be such that political movements attempting to profit from sentiments of national frustration, thus endangering the 'leading role' of the Communist Party, would not have the chance of successfully developing in any of the republics. The solution was found in a complicated give-something-to-all-and-take-something-from-all model. It balanced some of the inequalities existing between Yugoslav nations, but in return created new ones. Most importantly, it incorporated the calculated preservation of instability into the heart of the political system. The function of such system-generated instability was to legitimate the role

of Tito and of the party as the indispensable arbiters of national conflicts. The specific Yugoslav model of federalism was presented as incorporating a double negation – that of 'centralist' and 'unitarist' tendencies, generally attributed to the Serbs, and of 'separatism', usually identified with the Croats and Slovenes. The ideological core of the model was condensed in the rhetorical formula 'Brotherhood and Unity of Yugoslav Nations and Nationalities'. While accentuating the indisputable closeness of the 'Yugoslav peoples', the formula clearly preserved their 'ontological' separateness in sharp contrast to the pre-war ideology of integral Yugoslavism. The complex model inspired the creation of 'coalitions' between republics (principally Slovenia and Croatia, at times backed up by Bosnia and Herzegovina, Macedonia or the provinces of Kosovo and Vojvodina against Serbia proper and, eventually, Montenegro; for details, see Ramet 1992), which more or less successfully blocked Serbs as the largest nation in Yugoslavia from exercising any real domination, keeping the smaller nations relatively protected, if not content. Thus it could be argued that the only veritable dominator of the 'second Yugoslavia' from 1945 to its end was the party bureaucracy itself. What changed during the years were the extent of domination and the logic of its execution. In that sense, further 'developments' of the federal model, initiated by Tito in response to the challenges of nationalist outbursts during the 'the Croatian spring', produced the farthest-reaching consequences. Namely, the particular solutions adopted by the Yugoslav model of federalism, which after the constitutional amendments of 1971 and the adoption of the new constitution in 1974 could rightfully be described as strongly leaning towards confederalism, endangered by their decentralizing and atomizing logic both the efficacy of Yugoslav economy and the unity and the central position of the League of Communists (Koštunica 1988: 88; Goati 1989: 54). As far as the economy was concerned, instead of working towards the integration of a single economic system, the Yugoslav model produced six mutually competing economic mini-systems. Similar disintegrative tendencies were observable in the domain of politics. The weakening of the position of the League of Communists at the federal level was feverishly compensated by the strengthening of party organizations at the level of Yugoslav republics. Party organizations at the republican level legitimated the strengthening of their positions by presenting themselves as the defenders of political and economical interests of their respective republics. In that sense, they gradually took over vital segments of the political programmes of their worst enemies, the by then considerably weakened anti-communist nationalists. Appropriated also was their key mobilizing formula – the discourse unveiling economical, political and cultural victimization of one's nation by the devilish conspiracy of the Other.

Paradoxically indeed, the very mechanism envisaged for the prevention of national tensions that could lead to the dismantling of Yugoslavia became the strongest single factor working towards precisely that end. After 1974, the sole remaining federal institutions effectively functioning as guarantors of Yugoslav unity were the People's Army and Tito himself, already turned into an ossifying institution by excessive rhetoric devised for his glorification, rituals invented to give substance to the cult of his personality, and his age.

As can be imagined, paradoxical political developments had equally paradoxical consequences for the fate of ethnology. The newly discovered 'national mission' of party elites from the republics further strengthened the already mentioned tendency of ethnologists to restrict their research to 'their own' republics. On the other hand, because they increasingly identified with the role of guardians of national interests, party elites from all Yugoslav republics became jealously oversensitive to any public expression of concern for national issues independent of their control. Thus, while rapidly taking over important segments of nationalist programmes, party officials at the same time became ever more fervent critics of 'nationalist forces' in other republics, as well as of anti-communist nationalists in their own. For that reason, the concept of Nation in ethnological usage became sanctioned by even stronger ideological taboos. Thus, by the end of the 1970s ethnology in Yugoslavia and Serbia became more 'republican' (in the sense of being more enclosed within the borders of 'it's own' republic), but less 'ethnic' in the sense of evading any political engagement in the favour of one's own ethnicity, and finally more 'ideology bound' in the sense of being ready to obey to ideologically imposed 'tabooization' of certain themes like politics or religion, but less ideological in the sense of defending any ideology, and Marxism in particular. Of most importance, however, was the fact that due to rising professional standards and intensified intellectual contacts with American and European scholars and theories, Serbian ethnology also managed to become better informed and relatively more refined than it used to be in the previous decades.

The period brought forth the further development of studies of cultural change, the (re)invention of legal anthropology and a new impetus in the studies of material culture (see Prošić-Dvornić in this volume), and the gradual development of ethnicity studies (see Prelić in this volume). At the very beginning of the decade, Nikola Pavković and Djurdjica Petrović joined the Department of Ethnology[8]. Both researchers combined a strong interest in historical contextualizations with a readiness to cross disciplinary bounda-

[8] For a representative collection of innovative texts on various aspects of material culture, see Petrović 2003.

ries. Their research in the fields of legal ethnology and studies of material culture paved the way for a modernist, anthropologically oriented type of approach in Serbian ethnology, like the one later demonstrated by Mirjana Prošić-Dvornić in her path breaking historical-anthropological study of the relationship between the transformation of clothing and urbanization and Europeanization of nineteenth and early twentieth-century Belgrade (Prošić-Dvornić 1984 and 2006). Nikola Pavković, the first in his generation to enrol in a foreign doctoral programme (Paris, the Sorbonne), published his original study on the right of pre-emption in the customary law of Serbs and Croats in 1972. In it, he skilfully combined the methodology and sources of historiography with the theoretical frames of legal anthropology (*Pravo preče kupovine u običajnom pravu Srba i Hrvata. Studija iz pravne etnologije*).

As far as orienting ideas of Serbian ethnology from the 1970s are concerned, a rather heterogeneous set comprising traditional/popular culture, folklore, customary law, patriarchal tribal system, ethnogenesis, social change, material culture, industrialization, and urbanization took over the lead. The phase was marked by projects with titles like 'Continuous Observation of Transformations in Popular Culture' of the Ethnographic Institute of the Serbian Academy of Sciences and Arts, itself divided into two five-year periods, the first devoted to villages and the second to suburbs of industrial towns; by large interdisciplinary scientific conferences on key topics like customary law and communal self-management on the Balkans (Serbian Academy of Sciences and Arts, Balkanological Institute, Special Editions No. 1, Belgrade 1974) or on the methodology of ethnology (1974); by studies of the 'ethnogenesis' of Southern Slavs (Vukanović 1974), of 'ethno-anthropological problems' in Montenegro (Vlahović 1974), or of 'ethnogenetic processes' in Serbia (Vlahović 1976).

By the end of the decade, the inflow of concepts like Manifest and Latent Functions, Dysfunction, National Character (this time of the Culture and Personality brand) and Base Personality, *Rites de Passage*, Sign, Symbol, Structure, Binary Oppositions, or Generalized Reciprocity, signalled the development of novel orientations in ethnological thought in Serbia. Rather specific local brands of functionalism, structural functionalism and structuralism were gradually being developed. Functions of various social institutions were analysed, like those of the patron–client relations in the *kumstvo* (Rakić 1972); of traditional village fairs (Pavković 1972; Kovačević 1978); of the system of taboos in traditional Serbian culture (Bandić 1980); of collective village rituals (Bandić 1978; Pavković 1978); of religious forms of protection from various atmospheric disturbances (Kovačević 1976; Kovačević 1978); or of various elements of material culture (Prošić 1981; Bratić, Malešević 1982). Van Gennep's scheme was applied in the analysis of

carnival rituals (Prošić 1978); the breaking of objects as the marking of phases of wedding ritual (Kovačević 1979); taboo systems as regulators of social time (Bandić 1981); various urban rituals (Kovačević 1982); family rituals centred around the first menstruation (Malešević 1982); or mortuary rituals (Prošić-Dvornić 1982); the concept of post-mortem social death of a person (Bandić 1983); the socialist retirement farewell ceremony (Bratić 1984), or the initiation ceremony of young socialist pioneers (Malešević 1984; the ceremony of opening factory production lines (Antonijević 1986); the reconciliation ritual in blood feuds as rite of passage (Pavković 1993) and other examples. Attempts to apply structuralist methods became evident in the analysis of myths and village rituals (Kovačević 1974, 1978a, 1978b, 1981). An interest for similar topics provoked some local scholars to apply methods developed by the Russian semiotic school (Radenković 1978 and 1980). Finally, an 'ethnolinguistic' approach blending the ideas of R. Jakobson, D. Hymes, D. Ben-Amos, B. Bernstein, and R. Barthes was developed for the analysis of elements of mass culture in Serbia, be they socialist urban rituals celebrating 'the Day of Women' (Čolović 1972), or various genres of 'paraliterature' like epitaphs, obituaries in newspapers (Čolović 1974) or lyrics of the so-called 'newly composed' folk songs (Čolović 1982).

However, as far as the politics of ethnology is concerned, of utmost importance were path-breaking studies dealing with the pre-history of Serbian ethnology offered by Ivan Kovačević. In the first, 'The Influence of Romanticism on the Development of Our Ethnology' (1977), Kovačević critically analysed the central postulates of Serbian Romanticism as developed by Vuk Karadžić and his followers, pointed to the roles which such ideas had in the development of Serbian ethnology, and attempted to demonstrate that, contrary to what was generally taken for granted, their impact on the discipline was almost completely negative. By doing so, Kovačević attacked the very 'creation myths' of Serbian ethnology, the somewhat dusty but at the time still indisputable charisma of its 'founding fathers', and the 'sacred words' of the discipline like 'Custom' (Običaj), 'People' (Narod), or 'Psychic Types' (Psihički tipovi). What Kovačević attempted to demolish were the last remaining traces of the moribund dogma that only 'Our' particular 'autochthonous' way of doing ethnology was appropriate for the study of 'Our Culture' and 'Our People'. His devastating critique can also be understood as the coming into the open of a generational war that was already festering in the discipline for several years. As the war unfolded, several Gordian knots of Serbian ethnology became visible. The most interesting one could be associated with the side that Kovačević criticized, the elder 'empiricist' generation as represented by Barjaktarović and others. It was a generation of party members of rural descent, most, if not all of whom

associated communism and Tito's regime with their own chances of upward mobility, most of whom were educated and institutionalized just before or just after the war when the discipline was in decline, and which combined an un-scholarly and dogmatic loyalty to Marxism with an almost religious devotion to folk culture, romanticism and Vuk Karadžić as its 'patron saint'. The undisputed, though augustly far-off champion of this group was the earlier mentioned philosopher and ethnologist Dušan Nedeljković. As their adversaries, the romanticist Marxist empiricists had young urbanites, mostly from middle class, cultured and non-communist families, some of whom were educated outside the discipline (sociology or history of literature), who were fluent in at least one foreign language other than Russian, some of whom had spent some time abroad at a Western university or research centre, and all of whom upheld anthropology as their scientific standard and despised 'the science of grandmothers performed by grandfathers'. As is often the case with conflicts of identity that overlap with very real interests, this one was from the start destined to become a zero-sum game. The war was waged principally on two closely connected battlegrounds – the Ethnographic Institute and the Department of Ethnology. From the very start, the conflict appeared to have biblical connotations, pitting mature Goliaths against a bunch of frail David-like youths.[9] Quite expectedly, the battle was somewhat later decided by the slings and arrows of anthropological theory that the youths were expert at throwing in the faces of their angered but stunned adversaries (for reminiscences of this clash of generations, see Prošić-Dvornić in this volume).

Kovačević continued his revolutionary project in 1978 by publishing a programmatic text on the 'Semiological Approach to the Study of Rituals' (Semiološki pristup proučavanju obreda). Semiology, Structuralism and British reinventions of Van Gennep's *rites de passage* were to supply the main theoretical frameworks of the project that was intended to 'revolutionize' ethnology in Serbia, and direct it towards the standards of Anglo-Saxon and French anthropology. Thus, to follow but one example of systematically introduced theoretical innovations, in the relatively short lapse from 1978 to the second half of the eighties, Van Gennep's tripartite scheme was tirelessly applied in the analysis of carnival rituals, taboo systems as regulators of social time, various urban rituals, wedding rituals, family rituals centred around a girl's first menstruation, farewell ceremonies of young soldiers, mortuary rituals, and a number of socialist ceremonies related to school, family or factory (for a detailed account of the Van Gennep 'campaign' in

[9] For an illustration of the practical ways in which the war of generations was fought, see Dragoslav Antonijević's caustic review of Kovačević's paper on the relationships between exchange and gift in contemporary wedding rituals (Kovačević 1987: 41).

Serbian ethnology, see Kovačević 2006: 75–93). Van Gennep's scheme was intended to become the Trojan horse in the generational wars in Serbian ethnology during the eighties for three reasons. Its strategic aim was (1) to de-value methodologically the role of ethno-explication in the empiricists' approach to ethnology; (2) to bridge the rural-urban gap by demonstrating that what is truly important is the research method and not the location or theme of research; and (3) to demonstrate the overall superiority of 'scientific analysis' over 'village storytelling' with which the rival camp supposedly identified. It was a game of 'structuralism for beginners', in which the self-appointed adepts of the esoteric knowledge were very much confident that they would humiliate and defeat the unwilling beginners from the other camp.

Continuing his pursuit in his Ph.D. dissertation on 'Ethnology in Serbian Enlightenment' (Etnologija u srpskom prosvetiteljstvu, 1981), Kovačević offered an in-depth analysis of the ideas of Dositej Obradović and of other Serbian thinkers of the Enlightenment tradition, and added to his repertoire of sacrilegious postulates the thesis that Serbian ethnology could have had a better future had it chosen their ideas as its starting positions instead of those put forward by Vuk Karadžić. By so doing he reinvented a theoretical golden age for his project of voluntary anthropologization of Serbian ethnology.[10] In this alternative creation myth of Serbian ethnology, the romanticist illness of which the discipline was supposedly suffering could be cured by a radical 'jump back into the future'.

Taken together, Kovačević's studies can be interpreted as a theoretically refined re-stating of Kulišić's case against the conception of ethnology as a national and nation-building double-insider science, albeit from a different, non-Marxist and consciously anthropological position. Kovačević thus joined scholars like Manojlo Gluščević and Zagorka Golubović, who considered that (a) ethnology and anthropology were two distinct sciences stemming from differing intellectual traditions (Enlightenment and Romanticism), and that (b) ethnology was inferior both in domains of theory and research practices, and should be replaced by a revived Enlightenment-related anthropology, conceived as the general science of man. While such conceptions certainly strengthened emancipating trends in Serbian ethnology, they also brought a rather scholastic approach to the reflection on the identity of the discipline, and further contributed to its identity crisis (for

[10] Professor Kovačević recently supplied his own version of 'broad-stroke' retrospective analysis, in which he lays down his interpretation of the processes of anthropologization of Serbian ethnology (Kovačević 2006: 47–61). He concludes his analysis with a dose of anthropological triumphalism, pronouncing that the process was finalized as early as 1990.

details on Golubović's criticism of ethnology and her vision of anthropology, see Spasić in this volume).

Thus, the interaction of four factors – 'pragmatic' or 'technocratic' tendencies in politics, which induced the rising esteem of scientism; paradoxical outcomes in the domains of nationality policy and reforms of the federation, which resulted in the further weakening of the national voice in ethnology; further opening up towards the West with the theoretical influx it enabled; and finally the demolishing of some of the last remaining elements of romanticist heritage in ethnology – had a crucial, if perhaps unintended effect. Namely, the responsibility to ideals of science (or more precisely to a particular disciplinary conception of science) became the only surviving feeling of moral obligation in the discipline. In that sense, it can be said that between the end of the seventies and the second half of the eighties the double-insider syndrome (related both to national and to class loyalties) had definitively lost its hold over Serbian ethnology.

It was during the seventies that the Yugoslav socialist system began showing the signs of ideological exhaustion. As far as the topics addressed in this paper are concerned, the most interesting manifestation had to do with communist neo-traditionalism (Jowitt 1983). The trend manifested itself, among other things, in a renewed interest in the political potentials of popular and peasant culture. A good example is offered by the Cultural-Educational Community of Serbia (Kulturno-prosvetna zajednica Srbije), which in cooperation with local cultural-educational communities, began to organize in 1973 the manifestation formally entitled 'Competition of Villages of Serbia' (Takmičenje sela Srbije), otherwise better known as 'Village Encounters' (Susreti sela), which will remain the official channel of 'development of cultural life in rural communities' in Serbia for decades (for details, see Hofman 2007). The competition of local representatives was organized in the fields of folklore, vocal and instrumental groups, school choirs and other local amateur artistic groups. As a form of cultural activity inspired by socialist cultural policies, and aiming to counter 'retrograde ideas and lifestyles' and to create 'healthy socialist individuals', the Village Encounters can be compared to forms of politicization of folklore in other socialist countries, for example the nationwide festival 'Singing Romania' (Cintarea Romaniei, see Mihailescu in this volume). Once again, folklorists and ethnomusicologists got the better part of a politically significant project, unintentionally preserving ethnology from the spoils of power.

1980–1991: Even after Tito, Tito: Crisis and the Buildup to War

As could be deduced from the example of socialist neo-traditionalism, as exemplified in the form of 'Village Encounters', the eighties were going to

be everything but triumphant for Yugoslav socialism. The communist engine, thick smoke and loud noise nothwithstanding, had run out of ideological steam. In the beginning of 1980, it still seemed as if nothing radically new could happen in the coming years. Communism in Yugoslavia and Serbia seemed somewhat stagnant, but still stable enough to endure. However, antedating the *annus mirabilis* of 1989, three consecutive events each gave strong impetus to the disintegrative processes initiated by constitutional changes of the seventies.

One single concept – *crisis* – came to condense in the public discourse the complex and conflicting tendencies of the period (Goati 1989; Sekelj 1990). Namely, it was during the first half of the decade that pressed by the hard facts of life party officials finally had to openly confess that Yugoslavia was facing a 'profound economic crisis' and simultaneously a 'crisis of functioning of the political system'. In that sense, the eighties can be seen as *the phase of aggravating economic and political crisis*. Indicatively, declared to be in crisis was not the political system as such, but rather it's 'functioning'. Party officials preferred to keep silent on the profound crisis of the system's basic ideas and legitimization formulas, even though it was clear that more and more people were losing faith in the official narratives.

The three events that most profoundly shaped life in Serbia during the eighties all gave ample substance to the concept of crisis – Tito's death in 1980, the Albanian revolt in 1981, and the party *coup* by which Slobodan Milošević took complete control over the Serbian League of communists in 1987.

Nowadays, one perhaps recognizes some symbolism in the fact that two of the three events to which paramount political importance was accorded even at the time happened to be a burial and a birth. Namely, these events divided the period in much the same way as rites of passage do. The biological death of Josip Broz Tito in 1980 came to symbolize the end of an era, while the birth of Milošević's political charisma in 1987 can now be interpreted as the threshold of a new, extremely problematic and in the end tragic period. The liminal years left in-between were themselves marked by the Albanian revolt of 1981 and the resulting crisis in Kosovo and Metohija.

Tito's death represented much more than the symbolical ending of an era; after 'the greatest son of our peoples' was buried, the whole complicated system of calculated and controlled antagonisms between party elites from Yugoslav republics definitely blew apart as no formally 'independent' central authority was left to balance and legitimate it.[11] The horrid vacuum

[11] An alternative explanation is offered by Ramet's (1992) 'balance-of-power' model of Yugoslav federalism in which problems between republics are supposedly settled by 'internal' coalition shuffles without the need for an 'external' paramount arbiter. A strong argument

left after his disappearance was perhaps best expressed in the popular outcry of rhetorical magic making 'After Tito – Tito'. Entrenched into coalitions based on economic and political interests, but also cultural and religious (*sic*!) affinities, party elites lacked even the theoretical possibility of reaching consensus on issues relevant to the solution of key problems of the period. It can be said that Tito's burial finally unearthed all of the unresolved national questions in Yugoslavia. However, as the official doctrine still held that the nationalities question was solved in the best possible way, and as the legitimacy of the regime relied heavily on the credibility of such a doctrine, there were no institutional channels left for the resolving of such problems.

Thus, when the most profoundly disturbing political problem of the moment, the militant revolt of Albanian separatists in the Serbian province of Kosovo and Metohija exploded in 1981, party officials had no other option but to attach to it the absolutely misleading ideological label of 'counter-revolution'. Namely, as was obvious from the key Albanian mobilization slogan 'Kosovo – Republic', in crisis were issues of regional autonomy, minority rights, territorial solutions and unity of the state sanctioned by federal arrangements and the Serbian constitution, and not Socialism and the Revolution as such. The ideological definition of the problem as counter-revolution has led to its corresponding 'resolution' – to violent actions of the federal police and army against the rioters. Thus, Albanian protesters were martyred because of the communist regime's unwillingness to openly face the catastrophic consequences of the federal model and nationalities policy it had enforced. After the wreckage of their open demands for the removal of Kosovo from Serbia, the Albanian political elites took to the strategy of silently cleansing out Kosovo Serbs by constant social pressures and sporadic acts of terrorism (Petrović, Blagojević 1989). By choosing to do so, the leaders of the Albanian majority population in Kosovo profoundly frustrated the Serbs, both from the province and from Serbia proper, thus inculcating in them the belief that 'only a red-hot medicine can cure a burning wound'. Thus were created the necessary preconditions for the success of ruthless

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against Ramet's model resides in the fact that crucial political crises in Yugoslavia (1966, 1968, 1971) were almost exclusively resolved by Tito's (and that of his nearest entourage at the moment, like Edvard Kardelj) external arbitrage. Coalition strategies, while present during all phases of communist rule, gained prime importance as political decision-making procedures only after Tito definitively stepped out into history. It is important to note that the praxis of external arbitrage should not be confused with centralist initiatives in the federation. Rather, it can only function in a relatively decentralized and unstable context comprised of conflicting parties of varying strength.

political figures like Milošević and for the development of his repressive policies in Kosovo.[12]

From the beginning of the eighties a growing number of Serbian intellectuals devoted themselves to reflection on the 'historical price' of Serbia's 'Yugoslav adventure', to criticism of the League of Communists, which was held responsible for the country's 'grave economic and political crisis', and to presenting various possible solutions to 'the Serbian question'. The 'Memorandum' of the Serbian Academy of Sciences and Arts could be taken to represent the most elaborated voicing of such ideas (for the English translation of the original text and answers to criticisms by two co-authors of the text, see Mihailović and Krestić 1995). Contesting conceptions of regional autonomy and of minority rights in the context of fractured federalism and decomposing communism, strengthened by narratives of historical victimization, gave substance to Serbian nationalist grievances, which in turn further mobilized Slovene and Croat nationalism. Together, they lead to a spiral of mutual mistrust and an interactive dynamic of conflict building (Brubaker 1996).

In that way, instead of focusing on possible economical and political reforms, the public in Serbia, much as in other nationalizing Yugoslav republics, became absorbed by issues of national sovereignty, and of real or invented victimization by the 'malevolent Other'. Bitter fruits of such tendencies were to be tasted by many innocent victims from all warring sides in what became known, rather incorrectly, as the 'third round of Balkan wars'.

It is only when contrasted with the intensive nationalization of some strata of the Serbian intelligentsia during the eighties that the true scale of detachment of Serbian ethnology from the double-insider syndrome becomes fully apparent (Dragović-Soso 2002). The disengaged position of ethnology was most clearly visible in three key domains in which its politicization could have been expected.

First, in stark contrast to some of their colleagues from disciplines like historiography, linguistics or philosophy, who became leading men of the Serbian national renewal, ethnologists as individuals and even more so as members of a profession generally remained inactive on the 'national front'. Rare exceptions to the rule could perhaps be found in two articles by Radomir Rakić, 'Kosovo of the Serbian Nation' (1989) and 'Migrations and National Constituting of Serbs – A Proposal for Ethnological Research'

[12] Because of the 'post-history' euphoria related to the fall of communism, and to the processes of European integration and globalization, Western reflections on contemporary tragic dimensions of the Kosovo problem often neglect its historical spiral-of-violence dimension. The historically minded reader could start with Bataković 1991 and 1998.

(1990), basically because they treated central themes of Serbian national history with overt intellectual and emotional engagement.

Related to the 'national inactivity' of individual ethnologists was the even more important fact that during the eighties major ethnological institutions and organizations (Department of Ethnology of the Faculty of Philosophy, Ethnographic Museum in Belgrade, Ethnographic Institute of the Serbian Academy of Sciences and Arts, Ethnological Society of Serbia etc.) did not become significant centres of nationalist mobilization as did some other scientific or public institutions. There were, however, certain activities that could be seen as related to, or inspired by ongoing political processes. Thus, by organizing in 1989 the exhibition on 'The Legend of Kosovo in Folk Art and Tradition', the Ethnographic Museum in Belgrade joined the fervent activities around the celebration of the 600th anniversary of the battle of Kosovo, most of which were politically instrumentalized (Naumović 1994, see also Cvetković in this volume). Some projects of the Ethnographic Institute of the Serbian Academy of Sciences and Arts also focused on themes of 'national interest' (for example, the project 'Ethnological Study of Emigration from Serbia' which started in the beginning of the eighties, the conference on 'Contributions to the study of Ethnic Identity' held in 1989), but the approach to the study of such topics did not suffer from essentialism or nationalism as can be witnessed from the overall empiricist (or more correctly neo-positivist and even proto-constructivist) character of resulting studies (see for example Mirjana Pavlović 1990; Miroslava Lukić-Krstanović 1992).

Finally, contrary to what might have been expected, during the eighties ethnology did not supply Serbian political elites with any important body of 'nationally and politically applicable theory'. The point can perhaps best be illustrated by the writings of Serbian ethnologists on 'perilous' themes of strategic importance like ethnos and ethnicity (Bandić 1982; Vlahović 1982a, 1982b, 1983, 1987; Rakić 1983; Lukić-Krstanović 1986, 1988; Ljuboja 1988; Pavlović 1988; Petrović 1988a, 1988b; Prošić-Dvornić 1990; for a detailed survey, see Prelić in this volume). Thus, instead of supplying theoretical 'fuel' to general trends of ethnicization and nationalization of Serbian society as some proponents from other sciences did (some historians with stories of Serbs as victims of genocide), ethnological reflection on concepts like ethnos, ethnicity, or ethnic identity underwent further de-essentialization. Key theoretical references from the mentioned papers included works by E. Ericson, N. Glazer and D. Moynihan, A. L. Epstein, J. Bennet, F. Barth, A. D. Smith, W. Connor, A. Peterson-Royce, A. Anderson and J. Friders, but also S. M. Shirokogorov, or J. Bromlej, although to a considerably lesser extent. Due to refinements of theoretical perspectives,

ethnicity was no more related to mentality traits or psychic types (as in Cvijić's time), nor was it imagined as depending on firm, historically stable and inalienable sets of cultural traits, (as was done by some of the 'founding fathers' and by proponents of the 'ethnogenesis school'), but rather envisaged as a process to be interpreted and/or explained, or a 'strategy', as a distinguished author proposed (Prošić-Dvornić 1990).

On the other hand, as far as the 'active' aspect of the politics of ethnology in the eighties was concerned, the most important trends again did not have much to do with tumultuous political realities, but rather with internal disciplinary trends. Namely, sketched developments in the study of ethnicity were part of a wider modernist trend of theoretical refinement and thematic broadening of the discipline, which the eighties in part inherited from the seventies. Apart from the interest in problems of ethnicity, these trends principally manifested themselves in initiatives for the inclusion of elements of contemporary mass culture and of urban rituals into the repertoire of legitimate fields of ethnological study. The heirs of 'rural' ethnology of yesterday were struggling hard to turn their heritage into a modern and urban anthropology of contemporaneousness. Thus, shortcomings discerned in the up to then prevailing monographic approach were analysed (Nikolić 1989; Pantelić 1989); new directions of urban anthropology were traced; projects devoted to the study of contemporary myths and rituals of inhabitants of Belgrade were presented; novel types of urban rituals were analysed in a semiological perspective (Kovačević 1982); the change of meaning in the evolution of socialist rituals like the 8 of March from ideology to utopia was observed (Malešević 1988); various socialist ceremonies like the 'farewell of retired colleagues' were analysed; ardent analysis revealed that the secret of 'new customs' should be sought in 'old *bricolage*' (Pavković 1984); functionalist analysis was amended by a historical optic in order that a folk culture trait turning into folklorism could be followed from its original village context into the town (Ćupurdija 1981); the study of folklore was broadened to include 'mediatized folklore' – the analysis of folk tropes in the social communication through radio messages (Bratić 1883), or in intra-family communication via texts of 'newly composed folk songs' (Čolović 1982, 1984); novel elements of material culture like '*Gastarbeiter* houses' (Bratić, Malešević 1982) or monumental tombs (Kovačević 1985) were analysed as status symbols; myths surrounding blue jeans as contemporary commodity fetishes in a socialist country were 'demystified' (Kovačević 1980); the theme of women and of womanhood was opened and the opposition of masculine–feminine in Serbian culture analysed (Bandić 1985); patterns of ritual behaviour marking the sexual maturation of women were analysed in a communicational optic (Malešević 1985); finally, various types

of magazines for women were analysed in a historical perspective (Prošić-Dvornić 1985, 1986; Prelić 1989) and in the hope of unveiling value-systems and ideologies at work in them.

Thus, during the eighties the strange twist of the fate of Serbian ethnology fully revealed itself – the 'voice of the Nation' in what once used to be a 'national science' was finally silenced, but with it were silenced the last traces of more recent ideologies as well. Serbian ethnologists refused to fill in the echelons of nationalist intelligentsia, a remarkable feat for the heirs of a discipline whose founders considered the delineating of state borders as a task of prime importance. On the other hand, while Serbian ethnologists supplied prudent analyses that could have been used to counter the spreading of some of the inflaming narratives, these analyses remained on the pages of dormant volumes, as ethnologists generally refrained from presenting their ideas to the wider public, let alone to political leaders. Most importantly, because the discipline's initial identity became blurred, towards the end of the socialist era Serbian ethnology lost any consensual conception of what its social roles and responsibilities are, or should be. Thus, while there was nominal agreement that the discipline should be socially relevant, differences in the interpretation of what such relevance should in fact mean augmented. The great divide gradually came to be constructed around two key questions: (a) what should now be the legitimate topics of ethnological inquiry, and (b) should the discipline encourage or dissuade cultural, social, political or moral critiques of contemporary processes?

In reply to these questions, two basic positions were being developed from the end of the seventies to the second half of the eighties, and were consolidating themselves through constant clashes.[13] This should be understandable, as the positions were in particular profiled by the disciplinary 'cultural wars' from the eighties. With some simplification, the competing positions can be described as (a) *ethnological traditionalism*, and (b) *ethnological and/or anthropological modernism*. As was stated before, these positions were closely related to generational differences, so that among the traditionalists one would find people initiated into the profession at the beginning of the seventies or before, while the other camp consisted mostly, but not exclusively of those professionally initiated roughly at the beginning of the eighties.

[13] During the nineties a third position developed in response to the harsh political developments in Serbia - critically intoned and reflexive political anthropology (see Naumović 2002 and 2005). The traditionalist position gradually became restricted to museum curators, while the modernist position became so diversified that the term lost its classificatory value. The most provocative among such developments is the formation of a post postmodern approach, associated with the writings of Miloš Milenković (2003, 2007a, 2007b).

The first position somewhat unimaginatively re-invented the task of ethnology as 'the systematic study of folk culture, life and customs, ethnic and ethnological processes in Serbia, as well as of the Serbs living in Diaspora', and 'the study of contacts, permeation and other cultural relations between Serbian culture on the one side, and cultures of other ethnic groups on the other, over space and time' (Pantelić 1992: 21). Such a position clearly represented a consciously *traditionalist* response to the challenges of the period. In other words, it proposed the reinvigorating of the supposedly endangered basic traditions of a discipline facing identity crisis. The three central orienting ideas of this position: *Folk Culture and Traditions*, *People – Peasantry*, and *Ethnos – Ethnicity* decisively marked the professional production of scholars associated with it. The use of such *orienting ideas* should not be mistaken for simple intellectual inertia. Rather, it was to a large extent the result of a conscious effort to counter the effects of disciplinary identity crisis, the first signs of which appeared in the late sixties and became fully visible during the eighties. Furthermore, it aimed to prevent the spreading of tendencies that were considered undesirable, such as the 'anthropologization of ethnology'. In that sense, the politics of the approach should not be fully equated with the position characteristic of the 'phase of empiricism', but rather as an activist belief that in a situation of crisis revitalizing forces could be mobilized by returning to the founding ideas and themes of Serbian ethnology. The relevance of ethnology was in this perspective related to its status of expert science on a precisely defined set of topics supposedly having national interest. Its social aim should be to preserve the traditional core of national culture, not to change it.

The second or *modernist* position, initiated during the late seventies and considerably developed during the eighties and early nineties, focused on problems of disciplinary identity and the relations between ethnology and anthropology, on their theoretical foundations, diverging and converging trends, and in particular on the thematic widening of disciplinary fields of study. The modernists were more or less explicit in their desire to dispense with the last residues of the romanticist paradigm and to fully anthropologize Serbian ethnology. Scholars associated with the modernist camp were primarily interested in the study of processes of modernization, urbanization, material culture and everyday life patterns, as well as problems of ethnic and other types of identity. Researchers from the first generation of modernists developed a liking for historical methods, and worked towards interdisciplinary syntheses, like those offered by legal and historical anthropology, or the studies of material culture. Younger modernist authors were still ready to use concepts like manifest and latent function, but were also rapidly developing a liking for key words like structure, binary oppositions, process, strategy, or

context. The social relevance of ethnology/anthropology in the modernist perspective was related to the discipline's potential of contributing to the broadening of the scope of general knowledge, and thus also to the enlightenment and cultural emancipation of the wider public. The modernists believed that they could contribute to changing the world by changing the way we think about it. In Chris Hann's inspiring frame for the reflection on postsocialist anthropology in Eurasia, Serbian modernists could be designated as representatives of a nascent cosmopolitan anthropology (Hann 2003).

Conclusion

The first thing one can note at the end of this 'broad-stroke' retrospective of Serbian ethnology in the interesting times of Yugoslav socialism is that the story has no 'real' ending. The quick sketch of the clashing camps of ethnological traditionalists and modernists does not correspond in any epical way with the smashing-of-the-Wall image of the end of socialism.[14] Nothing comparable to the 'academic cleansing' that wiped out the careers of so many colleagues in what used to be the DDR happened in Serbia, neither in 1989/90, when formal multi-partism was introduced, nor in 2000, when Milošević's regime was finally overthrown by what some political scientists have called an electoral revolution (Bunce and Walchik 2006). New generations entered the field, novel positions were developed or reinvented in relation to emerging social and political realities, many of them tragic, fresh coalitions became engaged in previously existing or newly ignited disciplinary feuds, and the game became considerably more complex and interesting (more on such developments in Kovačević 2006). However, such a dynamic does not seem fundamentally different from the one that was emerging in the eighties, when the second generation of modernists was entering the disciplinary stage with sound and fury. Simply put, in Serbia there was no end-of-era phenomenon as far as ethnology/anthropology was concerned, apart from the disappointingly un-theatrical addition of 'anthropology' to the name of the Department of Ethnology. Have we then stumbled upon a never-ending disciplinary story?

[14] For an insight into some of the developments in Serbian ethnology after 1989 one could start with Kovačević 2005 and other papers in the volume on *Ethnology and Anthropology: Contemporary Standings and Perspectives* (Zbornik Etnografskog instituta 21, Beograd: Etnografski institut SANU, 2005), go through Bošković (2005 and 2007) for a caustic comparison of developments in Serbia, Croatia and Slovenia, and then go to Milenković (2003: 249-309) for a thought provoking reflection on the consequences of the crisis of representation for Serbian ethnology as a native anthropology.

One way to speculate about the 'anomaly' was offered by what might here be termed as *the failure of external causation hypothesis*. According to it, the reason for the lack of a suitable ending to the history of ethnology under socialism might lie in the fact that Serbia did not experience a 'real' political transition up to 2000, and that therefore there was no external deterministic mechanism available to spark off radically different developments in the discipline. An alternative *accomplished internal transformation hypothesis* argues that the process of anthropologization of ethnology (or internal struggle for that aim, more precisely) which was giving the discipline its dynamic from the late seventies onward came to be finalized roughly by 1990, and that thus was established the 'end of disciplinary history' (Kovačević 2006: 61). Romanticist legacy killed, and the status of anthropology once attained, there was nothing spectacular to wait for any more, just business as usual, new people, new theories, old game, in the best of all possible anthropological worlds. Beware, another Fukuyama-style never-ending story.

Both of these hypotheses possess a logic that is both seductive and distorting. The first one would want us to believe that disciplinary history is nothing but a function of external factors, so that absent a factor, absent will be its supposed consequences. But then, why is it that nothing spectacular happened in ethnology/anthropology in 2000, when democratization finally commenced in Serbia? The second hypothesis would like us to see anthropology as a Hegelian destiny to be fulfilled by what was once ethnology, and not as an unstable, rapidly fluctuating professional arrangement, the finality of which is attainable only in the eye of the beholder.

Even if these hypotheses were to be accepted, they would still offer only part of the required explanation. There are other realities that this story unveils, but they do not enforce coherence on the history of ethnology in the interesting times of socialism. I will try to briefly unpack these lessons from one relatively specific case of disciplinary history by playing with three popular tropes. What I hope to gain is insight into the ways in which socialism and ethnology interfered with each other, without Wolf's determinism from the turbulent mid seventies.

While socialism and ethnology lived side by side for half a century or more, depending on how one defines the Milošević era, their marriage was fully consumed only for a very short period. It was a rather brief encounter, historically speaking. But as often happens in failed marriages, the engaged sides seriously affect each other only after their 'flame' goes out. Socialism did not change ethnology in Serbia in any considerable way through revolutionary violence or fiery Marxist ideology, even though there were interesting and intense encounters on the way. What is proposed here is not the

thesis that 'strong power' has no real strength, but that it was deserted in the Yugoslav case before its effectiveness could be fully demonstrated. Namely, Yugoslav socialism did change Serbian ethnology, and in radical ways. However, in this particular case the changes were effectuated primarily by the skilful use of soft power, often with rather unintended consequences.

What the story of Serbian ethnology tells us next is that soft, invisible and informal power can be stronger in the long run than strong, visible and formal power. The skilful political use of soft, low-profiled power and its linking with informal networks in a discipline and its wider socio-political context may therefore constitute a very dangerous liaison.

Political and judicial repression, together with the ideological fury of Kulišić's denunciation texts from the sixties, all in themselves forms of strong power, in the end had little lasting effect on the fate of the discipline. Čajkanović was approaching the end of his career anyway, and Filipović could pretend to have never read the functionalists, and then diligently continue writing about the integrative social effects of reciprocity. Moreover, strong and openly expressed pressures and threats can strengthen the will to resist, help the formation of opposition coalitions, and in the long run inflict the loss of legitimacy upon those who wield power. The failure of the regime to silence Zagorka Golubović even after expelling her from the Faculty of Philosophy and after banning a chapter from her book speaks tellingly of the weakness of strong power under certain circumstances (for details, see Spasić in this volume). What the regime accomplished unwillingly was to boost her reputation and help her career as a visiting professor in the West an as a dissident at home. On the other hand, by not giving Filipović the opportunity to teach at the Department of Ethnology in Belgrade without causing any public scandal, the communists damaged the fate of Serbian ethnology for years to come and certainly prolonged the 'phase of empiricism'. All of that was accomplished without political costs. Here, the regime scored a clear point by relying on soft, socially invisible power. Filipović's case should be viewed as an element of a wider strategy, together with the fostering of folkloristics and the gradual and silent assigning of typically ethnological tasks to other disciplines like historiography, sociology, philosophy etc. This soft strategy of marginalisation of ethnology proved itself very effective. However, during the seventies and later on, unintended and even paradoxical effects of the strategy became observable, leading to what will be described below as one of the never-ending disciplinary stories.

Let us now briefly turn to the second type of soft power recognizable in our disciplinary (hi)story. The informal personal networks created by Nedeljković and inherited by Antonijević, skilfully masked by formal structures of the Academy of Sciences and of its Ethnographic and Balkanologi-

cal institutes silently interfered with the careers of many ethnologists, and in the end outlived the regime in the frame of which they were created. I still vividly remember being invited by Antonijević for an informal interview during which an indecent proposal in the disguise of collegial advice was put forward to me. According to the proposal, I would have a paper of mine published if I were to take out of it passages which (a) related Vuk Karadžić to Serbian nationalism, and (b) described the logic of political instrumentalization of elements of Serbian national tradition by Milošević's regime. Such things, I was then told, could not be published 'under the hat of the Academy.' I was made to understand that should I accept, more friendly advice would follow, leading to more of my 'correctly written' papers being published 'under that hat'. A prestigious institution was thus instrumentalised in an attempt to broaden the reach of personal client-networks. The text eventually did come out, but several years later, and in a volume edited by Professor Mirjana Prošić-Dvornić (1994). If one was pessimistic enough, one could think of such dangerous liaisons as being a never-ending story in many a discipline around the world. In this case, however, they were an undeniable result of the intertwining of party-politics and personalised networks of clientism.

I take the missing end of the story of Serbian ethnology in the interesting times of socialism to be one of the indicators which point to the 'never-ending stuff' that is revealed by the story. While I believe the 'stuff' to be abundant, I will concentrate only on two examples.

First example. One additional way to explain the 'missing end' is to postulate a third hypothesis according to which once a discipline is politically and socially marginalized, which seems to have happened to Serbian ethnology under socialism at a relatively early stage, it becomes very difficult for the powers to keep control over it. Marginalized groups develop internal dynamisms, while attempting to sever the ties that bond their members to the circles that have excluded them. In time, they become self-absorbed, getting fully out of reach of those who attempted to influence their activities. In brief, the first never-ending stuff in Serbian ethnology, at least from the advent of socialism to nowadays is the at first enforced and later on self-sustained marginality. Namely, marginality as a state of things ethnological, even though induced by socialism, acquired a logic that helped it outlive the system. Furthermore, as long as the wider community and administrative and political elites in particular remain disinterested in ethnology as a discipline, and as long as its practitioners remain content with their marginal position, the discipline will retain its autonomy, and will stay relatively immune to external influences and pressures, no matter how dynamic they are. Paradoxically enough, the politically motivated effort to minimise the

social relevance of ethnology is exactly what in the long run gave the discipline it strength, its originality, and its creative potential. As Eric Wolf put it while explaining how anthropology gained and preserved its particular perspective: 'that what limited our success among the powers constitutes anthropology's strong point, and distinguishes it from its more prominent sister disciplines' (Wolf 1999: 132).

Second example. Before, during, and after socialism, Serbian ethnology functioned, to put it jokingly, as a 'science of the natives, by the natives, and occasionally for the natives'. To say this, however, is not to imply that it remained a national science from the time of its institutionalization and professionalization all the way to today (Naumović 2005). Serbian ethnology stopped functioning as a national science quite some time ago, and successfully resisted the siren calls that were tempting it to return to its national mission during the nineties. However, it remained restricted by the 'Own' in two ways. First, during the whole of its history, the study of geographically distant cultures was absolutely marginal to the tradition of Serbian ethnology. Second, because of that fact, long-term fieldwork itself remained marginal in it. To become a formally accepted member of the ethnological profession in Serbia, one now does not need to spend a certain number of months in the field collecting data about people who live far away, do things differently, and speak a different language. Thus two key constitutive features of the anthropological tradition remained, and still remain external to the tradition of Serbian ethnology recently renamed as anthropology. Socialism changed nothing about this never-ending story, apart from making it even more absurd by enclosing what used to be Yugoslavs in their respective republics. Therefore, the cherished internal disciplinary narrative on the anthropologization of Serbian ethnology hits hard against the never-ending story of the discipline's enclosure in the sphere of the own, and its reluctance to engage in long term systematic observation of other people's behaviour. This, however, does not mean that there were no substantial changes. From the seventies onwards, and even before that, anthropological theories were more or less successfully internalized by several generations of Serbian ethnologists, and they were and are ever more intensively applied in local research. But is the growing corpus of theory the only characteristic that makes anthropology the discipline that it desires to be? However, to open that question is to slip into another never-ending story, this time called 'what is anthropology?'

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Chapter 12

Ethnographical Institute of the Serbian Academy of Sciences and Arts (SASA) in Search of the Lost Subject of Serbian Ethnology: From Ethnos to Ethnic Identity

Mladena Prelić

Introduction

In the context of the new, socialist, federal state, the 'science of people' – ethnology, as defined in the previous (civil) society, remained not only without its earlier defined subject (*ethnos*), but also without its social goal (determination of its own ethnic and national boundaries and definition of its own national culture). This paper is about the reactions of ethnology in Serbia to this situation – about theoretical–methodological changes in approach to ethnic phenomena in the period of socialism, as well as about attempts to define the subject, and indirectly also the social goals of ethnological research, in new ways.

The paper first offers a brief description of the socio-political context of the surrounding society, particularly considering the political organization of ethnic and national differences, followed by changes and development in definition of ethnic phenomena (from ethnos, ethnogenesis, to ethnic identity), and concludes with a presentation of the case of the Ethnographic Institute, which, since its foundation has been defined as the main institutional framework for dealing with ethnology in a scientific manner, and which can serve as a paradigm for the entire branch – for its search for the (lost) subject and social role in the period of socialism, as well as for its marginalization.

Context: The Political Organization of Ethnic and National Differences in the Second Yugoslavia

Yugoslavia was founded in 1918, as the kingdom of Serbs, Croats and Slovenes. In 1929, its name was changed to Yugoslavia, and from then until World War II the official ideology of an 'integral', unitary Yugoslavism

prevailed. The 'second', socialist, Federal Peoples Republic of Yugoslavia, which was formed during and after World War II, was an entirely different state both in its form and its content. The idea of federalism, as the only possible solution for the harmonic living of an ethnically, nationally, religiously and linguistically diversified population, started, at first unsteadily and with internal debates, to formulate itself within the Communist Party of Yugoslavia in the 1920s, and to obtain its full legitimacy during World War II (Djilas 1990: 79–117). It was, at the same time, an anti-fascist resistance, a social revolution and a civil war. Upon foundation of the new state, the first Constitution, adopted in 1946, took over the Soviet model in many of its solutions, among which also in the issue of a federal structure, but in a simplified form, without the complex, multiple Soviet system of federal and autonomous units. During the war, five constitutional nations of the future state were determined (Serbs, Croats, Slovenians, Macedonians, Montenegrins) and six federal units (in addition to the mentioned five nations which realized continuity or establishment of their national states through federal units, Bosnia and Herzegovina was created, reflecting, in some sense, a still unresolved 'Muslim issue'): later on, two autonomous provinces (Vojvodina and Kosovo) were added to Serbia. In many ways, however, in practice, this system was a negation of the real federal principle (which in its democratic mode should reflect the principle of autonomy of federal units); it was, paradoxically, a centralistic, party federalism (Tošić 1997: 49–72).

In the years that followed, the system was often reformed, which was explained as a search for its 'own way to socialism', by creating an allegedly unique, superior social model, 'better than East and West'.[1] In reality, during the course of time this system yielded national, extremely bureaucratic, party structures, which during the 1980s placed themselves at the head of currents that led toward the dissolution of the country. The paradoxical mode of a centralistic federalism that the communists founded after coming to power was destroyed by their attempts to carry out the Marxist idea of the vanishing state. However, the reforms only paralysed the federal state, but not its constitutional units (republics), which in time advanced to full statehood (Tošić 1997: 60–7).

In socialist Yugoslavia, nationalism and socialism (communism) had an ambivalent relationship. The national principle was perceived dually, as in the distinction 'national/nationalistic', as positive and negative. 'National' was something that had to find its place in the new union of Yugoslav peoples – for national is not only bourgeois but also a legitimate, 'legal' way to emancipate the people – which includes both workers and peasants (social-

[1] After the conflict with Stalin in 1948, Yugoslavia was not a part of the Soviet block.

ism encompasses the national liberation of oppressed small nations, as well as the social liberation of oppressed classes),[2] whereas 'nationalistic' (understood as bourgeois, imperialistic) was a source of danger for the survival of Yugoslavia.

The communists envisaged Yugoslavism not as a melting pot,[3] nor as a unitary Yugoslavism under which Great Serbian nationalism was hidden (although such a fear existed in other members of the federation), hence, not as a new cultural, ethnic or supra-national identity, which would fuse separate national identities, but as a political identity of a 'self-managing, socialist man'.[4] The possibility was nevertheless left for Yugoslavs to form a new, unique nation one day, but party officials always emphasized that it had not to be imposed, but a spontaneous solution, which is the matter for the future. The meaning of Yugoslavism was largely discussed in public during the 1980s – in 1981, when for the first time it was introduced in the census, as a category for Nationality, and then throughout the development of the social and political crisis opened up by death of Josip Broz and changes of the bipolar global system ('destruction of communism').[5]

The position of minorities, which after the Constitution of 1963 were defined by the term 'nationalities', was another field of party control, but at the same time with very successful solutions for balancing internal differences and complexities. A very favourable position of national minorities in the then state was used for propagandistic purposes as well. That does not mean that all implemented solutions were bad, however. Although totalitarian by definition, the Yugoslav government attempted to obtain the peoples' approval and to certain extent it governed in the common interest or found ways to achieve voluntary acceptance by a large number of citizens. The contents of the 'solutions superior than both East and West' were often democratic. The problem was that they were brought in without a democratic procedure and applied arbitrarily.

'Large-scale balances' in foreign and internal policies of the second Yugoslavia were a reflection of great political skill, but in the long run they simply covered up the actual contradictions in the system, instead of opening up ways for their resolution. Balancing, and also a 'controlled' allowing, and

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[2] Besides Marxism classics, social theoreticians of the second Yugoslavia in this area particularly relied on the work of Edvard Kardelj, who was considered the main ideologist of the Yugoslav social system. See, for example, Tribina (1981: 101–11).

[3] In some opinions, the period 1960–4 is exempt (Bataković 1995).

[4] In this case too, the ideologist of such a concept was Edvard Kardelj (cited by Tošić 1997: 30). We can compare this formulation with the Soviet one – national in form, socialist in content.

[5] About Yugoslavism as a specific socialist patriotism, see Janjić (1988: 189–95).

even encouraging, national conflicts by the highest level of government was on several occasions used as a means of manipulation, whereas that same head later appeared as a guarantor of stability and established order from the highest position (Bataković 1995). In this way it hindered participants in the federation from learning to resolve their own relationships in democratic procedures. Until the end of the 1980s, party bureaucracies developed in national bureaucracies of six states and two provinces with almost all elements of statehood. When the framing ideology disappeared, together with the 'guarantor of stability' himself, there was nothing left to keep the union together. The 'brotherhood – unity' ideal claimed by so many party forums turned to empty slogans.

The roots of the belief that the national issue in the second Yugoslavia had been solved reach back to the time of the World War II. That belief was fed by a real historic success in stopping mutual massacres and opening up a perspective of living together in an ethnically and nationally complex state (Tošić 1997: 19–23). This was the basis on which the party in power grounded its legitimacy, and even its popularity among many citizens. Actual establishment of democratic federalism, and open discussions about national relations, conflicts, problems, historical heritage of national identifications, were, however, missing (*ibid.*: 31–2). Since they were of essential importance for the very foundation of the new government, ethnicity, nation and nationalism in fact became a taboo (although covered by party controlled, ritual rhetoric). The central powerholders in the party determined who and in what ways one could express national identity and speak about it.[6]

From Ethnos to Ethnic Identity

Ethnology in Serbia before World War II can most concisely be defined as a science of people (*ethnos*). One of the most influential Serbian ethnologists before the second world war defined ethnology as a science of the peoples of the world, which studies the origin and development of each group and all characteristics and phenomena that make it a nation, with a final task of finding regularities in the ethnic development of mankind (Erdeljanović 1939: 3). By its definition, ethnos represented the key term of ethnology, while culture was also researched, although not as a phenomenon in itself, but as an inventory of ethnic distinctions or even essence of ethnos. As a 'national science', ethnology had defined social goals – to create (collect, select, study) contents of national culture/heritage, and to define the boundaries between *us* and *others* on the basis of the ethnic distinction manifested

[6] See, for example Bringa (1993), for the case of Muslims in Bosnia.

through culture (Kovačević 2001: 7–26). Interfering with politics, not only on the level of scientific goals but also on a personal level, was reflected also in the fact that some of the 'founding fathers' of Serbian ethnology had prominent political roles and careers (Jovan Cvijić, Tihomir Djordjević, among others).

The end of World War II brought a radical cut – the value system of civil society, and that society itself, were replaced by something new and different. As we briefly showed in the previous section, the political and social organization of ethnic differences underwent profound changes, and therefore, somewhat defined the social role of ethnology. Under these new circumstances, what had happened so far with ethnos as the founding term of this science?

If we speak about ethnos, as a presumed key term of ethnology, it long remained an implicit concept, not only in Serbian ethnology. The first explicit request to study ethnic phenomena as defined scientific problems came from Milenko Filipović in the text published in 1961 – *Stvaranje etničkih grupa na planinama* [*Creation of Ethnic Groups on Mountains*]. It is not unusual that it came so late – even in more serious ethnological and anthropological traditions the terms of ethnic group, ethnicity or ethnic were tacitly understood, but not defined or named as research problems. In the case of the former Yugoslavia, there was an already mentioned, additional reason: ethnic matters were considered a sensitive issue – there was a danger that its research and problematization might disturb a carefully maintained balance and proclaimed non-problematic situation ('solved national issue').

In the cited text, Filipović cites Sirokogorov as the sole creator of the theory of ethnos, although indirectly, through Carl Dittmar, and then he himself defines ethnos, i.e. ethnic group, as any human group, regardless of its size, which through its way of life and its views is distinct from other groups of the same range. Ambiguity of this term lays in the fact that at the same time it pertains to the group itself in its cultural specificity and to culture/cultural creations which that group produces. In Filipović's opinion, ethnologists should deal with both, but primarily with ethnic groups and determination of their specificities, and only after that with culture, which is a universal human characteristic.

Filipović's work *Creation of Ethnic Groups on Mountains* presents three examples of small ethnic groups which had formed and then disintegrated under specific circumstances, with these circumstances taken very broadly – from geography (configuration of soil, regional characteristics) to politics (influence of government). It is interesting, however, to note that in Filipović's opinion it is the ethnic group, and not the culture, that survives as the main subject of interest in ethnology.

It seems that Filipović's call for a theoretically aware approach to ethnic phenomena did not have direct results, but that same year, for example, a long article by Mirko Barjaktarević – *Peroj i njegovi stanovnici* [*Peroj and its Inhabitants*] was published, so one should compare the two works: Peroj is a small place in Istria near Pula, a colony of Crmnics – inhabitants of the southern Montenegrin coast from the seventeenth century – which moved to Istria and preserved itself as an endogamy and orthodox oasis in spite of its small size. Historical context was mentioned, but it remained unlinked with the essence of analysis. Also, the context of the surrounding society was mentioned – communication with the surrounding people – however, the role of any external or historical factors in the preservation of the group was minimized and left at the level of description. This text is characterized by its search for 'authenticity', for continuity of cultural patterns, important in themselves, with explicit expression of scorn towards foreign cultural imports, that which is not 'ours', 'folk', as well as towards ethnologists who in the author's opinion search using all means for foreign influences on 'our', 'domestic' culture (Barjaktarović 1961: 86). Ethnology is, therefore, in this case still understood as 'our science about us', intolerant of deconstructionist threats.

The term of ethnos, which was implicitly operated with at this time often tacitly keeps its romantic content.[7] Besides, during the 1960s, the anthropological approach founded by the work of Jovan Cvijić was still considered highly relevant as a theoretical frame of field research of ethnic phenomena. It meant orientation toward issues of origin of population, migrations, distribution of ethnic groups and their dependence on geographic factors. It is interesting to note that works of ethnologists who rely on anthropogeography use terms such as *ethnic unit* and *ethnic border*: under the term of ethnic unit they understand geographic units with certain cultural specificities, within which the population intermingle and thus in perspective more and more assimilate, regardless of the lack of ethnonym, or individual awareness of ethnic specificity of that population, and its factual origin.[8]

Among else, the concept of the anthropogeographic works of ethnologists of that time envisaged an ambitious goal – to register, i.e. to study, the origin and moving of each family within the researched area (Antonijević 1971: 15), on the basis of written sources, but, in their absence, and due to ethnographical uncritical overestimation of 'oral sources', often on the basis of memories of conversations in the field. The aim of such research, foreseen to be carried out systematically all over Serbia, was the reconstruction of

[7] For example in the concept of *folk spirit* of D. Nedeljković.

[8] Antonijević 1971. It should also be noted that according to this concept ethnic and geographic borders do not necessarily match (*ibid*.: 11).

ethnogenesis of the Serbian nation and other ethnic groups living in Serbia. Overwhelmed with data, some of which was often uncertain, ethnologists of anthropological orientation were, however, slowly losing the inspiration of their great predecessor, and particularly his capability for synthesis, so that before the end of 1970s anthropogeography returned under the wing of geography, from where it had originated.

About ten years after Filipović's initiative, albeit with certain caution, ethnic phenomena gradually become a separate, consciously considered subject of study of Serbian ethnology. Here a large role was played by Petar Vlahović, professor of the Group for Ethnology[9] and Dušan Drljača, Associate of the EI SASA.[10] Vlahović's works on ethnic problems can be divided into four groups – (1) his own research – particularly in Serbian-Muslim surroundings in Sandžak and Montenegro; (2) a whole series of small contributions presenting Soviet theories of ethnos and ethnogenesis; then (3) ethnogenesis; and (4) physical anthropology and anthropogenesis. As far as the import of Soviet theories is concerned – those of Sirokogorov, Bromlej – it should be said that this was primarily reduced to presentation – in his own research, Filipović partly relies on these theories, but without applying them in full.

Dušan Drljača[11] particularly engaged in the problems of small ethnic groups, emigration and Serbs in the diaspora (particularly ethnic processes, migrations, cultural merging). In addition to a serial of papers with ethnic topics, outstanding is a monograph on colonization and life of Poles in Yugoslavia (1985).

The reception of Soviet scientists in our ethnology remained relatively weak and superficial. No paper of Bromlej has ever been translated to any of South Slavic languages; only a short article, dealing just with the term of ethnos, was published in Russian language in Zagreb in 1971 (Bromlej 1971). This illustrates the low-level reception of his works, without nevertheless affecting the application of his theory. In 1980s, Bromlej was eventually cited in literature and mentioned in university lectures as the 'creator of the sole theory of ethnos so far'. Besides Vlahović's, there were almost no

[9] From 1955 an assistant, and then assistant professor and professor of the Department of Ethnology (and Anthropology). Among other things, starting from 1974–5 he introduced and taught the course on Ethnogenesis of the Peoples of Yugoslavia.

[10] It is interesting that both scientists come from an ethnically mixed, prevailingly Muslim-Serbian environment – Prijepolje and Bihać – which might have induced their interest and receptivity for these issues in their youth.

[11] Since 1960 associate of the Ethnographical Institute of the SASA, today a retired scientific advisor.

other interpretations of this theory.[12] Bromlej's theory was known, but never strictly applied.

For the sake of a comprehensive picture of the study of ethnic phenomena in this period, we should turn to the problem of ethnogenesis as well. In Yugoslav ethnology, only three synthetic works on this topic were published: Niko Županić, *Etnogeneza Jugoslovena* [*Ethnogenesis of Yugoslavs*, 1920], Tatomir Vukanović, *Etnogeneza Južnih Slovena* [*Ethnogenesis of Southern Slavs*, 1974] and Špiro Kulišić, *Etnogeneza Crnogoraca* [*Ethnogenesis of Montenegrins*, 1980].

Why was ethnogenesis suppressed and neglected in ethnology? What should be noted first, is that it was not neglected just in ethnology – some experts speak about a period of 'ethnogenesisophobia' in the time of socialism (Jevtić 2003: 48). There are several possible answers: in order not to disturb the 'South Slavic myth' (related to this is some scientists' intolerance of the issue of the 'Vallachian component' in ethnogenesis of the Southern Slavs), or because of ethnogenesis' insufficient scientific foundation, due to a lack of historical sources; in the case of ethnology we can assume also the inadequacy of those ('oral') sources that are considered as specific to ethnology – ethnogenesis was to a larger extent the field of archeologists and historians dealing with the Middle Ages, and philologers–language historians, but in neither of these cases had it an excessively high social status in the period of socialism. The third, and the most probable assumption, is that it was suppressed as it had been perceived as a possibility of national mythologization of individual nations on ethnic grounds – what the party considered as a re-ethnification of nation,

In relation to the book – *Ethnogenesis of Montenegrins* by Špiro Kulišić, at the time of publishing (1980), there came a strong politicization of debates about ethnic phenomena, which stirred up spirits and provoked a whole series of scientific, but primarily political debates. In his book, Kulišić claimed that the origin of the Montenegrins is not entirely South Slavic, hence ethnically they completely differ from Serbs. The direct political implications of this text and the political debates that followed, linked politics, ethnology and ideology. Ethnology as a science was now more present in public, but it is obvious that this debate was marked by a strong political–ideological frame – although it called for the authority of science and the need for a non-ideologized scientific debate and argumentation, it essentially discussed the possible political implications of Kulišić's claims. Party forums insisted on the 'correctly understood' problem of ethnicity – ethnogenesis, above all in an ideological sense (Tribina 1981).

[12] An exception is the work of Gordana Ljuboja (1988).

It is interesting, however, that already from the mid 1970s ethnologists in Serbia had been interested in the phenomenon of nation and the possibilities for ethnic study (for example, M. Draškić 1975; P. Vlahović 1978, 1983). It should be noted that 'nation' is understood as a part of an evolutionary series: ethnos – people – nation, but also that the 'ethnological approach' to this issue remains marginal when compared to the work of political scientists and sociologists, particularly those who dealt not within the framework of scientific institutions, but of party forums.

During the 1980s, ethnology made an obvious step in the process of anthropologization of the study of ethnic phenomena. Its meaning may be resumed by the terminological shift from ethnos to ethnic identity

An important text dealing with theoretical definition of ethnos (still terminologically defined in that way), 'Etnos' by Dušan Bandić (1982), cites Soviet theories, but is essentially on the path of Barth's understanding of ethnicity as a subjective feeling of belonging notwithstanding 'objective' cultural contents.

Debates in the magazine *Gledišta*, where in the early 1980s various social scientists spoke about issues of definition and ways of studying 'ethnos – people – nation', made another move toward the theoretical understanding of ethnic phenomena from the perspective of ethnologists – Radomir Rakić (1983) proposed that by ethnos we should not understand a group but a type of social connection based on (imagined) blood relations. By intuitive and almost incidental remarks about the nature of ethnic phenomena, these two papers introduce us to their modern understanding and interpretation.

The beginnings of more systematic research of ethnic phenomena in the context of the then cultural anthropology are related to the reception of Fredrik Barth's work, which came through Croatian ethnology.[13] Works of several ethnologists towards the end of the 1980s[14] announced a more serious theoretical foundation for studying ethnic problems in the spirit of cultural anthropology of that time.[15] Work of Edit Petrović and her research of Montenegrin colonists in Vojvodina should be particularly emphasized. The 1980s brought also theoretical–methodological improvements in the study of ethnicity carried out in two directions within the projects of the SASA Ethnographical Institute: the study of minorities in Serbia and of the

[13] First of all, Auguštin Dunja Rihtman (1983) and Olga Supek (1988).

[14] Gordana Ljuboja 1988; Edit Petrović 1988a, 1988b, 1989, 1990; Mirjana Prošić-Dvornić 1990.

[15] They relied on theories formulated until the end of 1960s for example in papers of F. Barth 1969; A. Cohen 1974; G. De Vos 1975; Glazer and Moynihan 1975; Epstein 1978 etc.

Serbian diaspora.[16] Since the end of the 1980s, ethnos, ethnic groups and ethnic process have been less and less described as stable, objective categories, and more and more as ethnic identity/identities, i.e. subjective, flexible, symbolic processes of ethnic identifications.

The Work of the Ethnographical Institute of the SASA

The Ethnographical Institute of the SASA has since its foundation been defined as a central scientific institution dealing with scientific research work in the field of ethnology in Serbia.[17] Scientific institutes in Serbia and Yugoslavia were systematically established after World War II. Preparations had already started in 1945, and Institutes were established by decrees of the Committee for Universities, Advanced Schools and Scientific Institutions, a body within the Government of Serbia, during 1946, 1947 and 1948.[18] The case of the Ethnographical Institute is, however, somewhat ambivalent: the initiative came from the then President of the SAS, who was at the same time the President of the Committee on Ethnography, linguist Aleksandar Belić. The decision to establish the Institute was taken in the Academy (Committee on Philosophy) where the proposal had been drafted. A governmental Decree of Establishment was issued in June 1947. The Statute was brought on the joint session of the SAS and the Government's Committee (D. Zečević 1952: 577–8). Two themes are seen to interweave both in the

[16] Particularly the latter field saw theoretical–methodological progress in works of Mirjana Pavlović and Miroslava Lukić-Krstanović, of which more will be said below.

[17] Institutions in Serbia dealing with ethnology in various scope and in different ways in the observed period were, for example: Department for Ethnology of the Faculty of Philosophy in Belgrade, the Committee on Social Science, Committee on Rural Areas and Committee on Ethnic and National Minorities and Human Rights of the SASA, the Institute for Balkan Studies of the SASA, the Ethnographic Museum in Belgrade, local and homeland museums in the Serbia countryside and in provinces, Matica Srpska in Novi Sad, the institutes of some national minorities, for example Institute of Hungarology in Novi Sad, museums of religious and ethnic communities, such as the Jewish Museum in Belgrade, etc.

[18] It can be assumed that the definition of the role of the Academies of Sciences and institutes in their composition was done by Soviet model. Some Academies of Sciences in the then Yugoslavia were, however, founded in the second half of the nineteenth century, SASA and YASA in 1866 (SASA first as a Serbian Learned Society, than since 1886 as a Serbian Royal Academy, since 1945 SAS and then SASA), and Slovenian Academy of Sciences and Arts in 1938. Up to World War II in Yugoslavia there were 215 scientific research institutions, out of which 27 were from the field of social sciences, mostly linked to universities. Since 1945, the new state invested in the founding and development of universities and institutes and the increase of the number of scientists, considering it as a condition for general social development. From 1945 to 1956, the number of faculty scientific institutions was multiplied, while the number of extra-faculty ones increased from pre-war 15 to 252. Of these institutes, 67 dealt with social sciences (MPE, 1968).

organizational sense and in the sense of setting goals and tasks between the Government and the Academy – the 'new time' and the continuity since the end of the nineteenth century: the formal foundation act, and a part of set goals and financial support both came from new government, but, on the other hand, the structure and a part of the defined goals were inherited from the nineteenth century – the Institute was considered to be the successor of the Committee on Ethnography and the *Serbian Ethnographic Journal*.[19]

As the first director of the Ethnographical Institute of the SAS noted, although that institution was founded in the new era and under the conditions resulting from the victory of revolution, it 'by its task and composition, achieved historical development, carries in itself three main components of an earlier scientific achievement of the SAS, which started to be built already six decades ago – the *Serbian Ethnographic Journal*' (after which the Institute was named) (Radovanović 1952: XIII–XIV).[20] It is interesting to note that the very term of ethnography, designating continuity with the nineteenth century, at the same time also happily fitted into the scientific tradition of Soviet ethnography.

The first Statute of this 1947 institution stipulated that the task of the Institute was to 'organize the systematic and planned investigation of settlement and the origins of the population, folk life and customs, as well as of folklore in our country and among our people outside Yugoslavia'. Three directions of research which were defined then correspond in fact to the division of the *SEJ* in three departments – Settlements and Population, Life and Customs, Folklore (later added was the fourth department of the *SEJ* – Debates and Funds). Accordingly, the Institute from the beginning had anthropogeographic, ethnologic and folklore departments. Within the anthropogeographic department already in 1950 a Department of Folk Architecture was founded, and in 1953 a Department for Sociology of Settlements, whose task was 'to study society in urban and rural areas through phases of our socialist building' (Urošević 1972: 6–8).

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[19] The initiative for launching the *SEJ* belongs to Stojan Novaković (1892). The *SEJ* was first published in 1894. It had four sections: (1) Settlements and population; (2) Folk life and customs; (3) Folk creations; and (4) Debates and funds. At the head of the *SEJ* as a scientific–organizational body within the SASA (than SRA) was the Ethnographic Department with its Committee on Ethnography (from 1898). The last to preside over the Committee from 1927 to 1947 after Ljubomir Stojanović and Jovan Cvijić was the then President of the SAS, Dr. Aleksandar Belić (Radovanović 1952: XIII–XIV).

[20] Publishing of *the SEJ* was since then taken over by the Department of Social Sciences which within the Academy was also in charge for the Ethnographical Institute. Publishing of the *SEJ* was continued until the end of the 1970s, and than it spontaneously (although not formally) stopped.

To sum up what has been discussed so far, it can be said that the goal of the Institute, defined by the Government and by the Academy, was the research of populations, settlements, migrations, folk life and folklore. When observing the published results – it seems that 'folk life' (understood in sense of social life, everyday life, popular culture, folk religion etc.) was pretty much neglected among these problems.[21] However, within the framework of that segment, clothes and architecture were studied the most. The reason for this neglect could be the ambivalent attitude of the communist government toward the village, the situation in villages in the1950s (collectivization, forced purchase, often against the will of the peasants), as well as a negative attitude towards tradition and religion. It can be assumed that it would not be acceptable for a new government to deal with such topics. At that time, anthropogeographic studies were encouraged, as well as the collection of partisan and new socialist folklore. Under the leadership of Prof. Dušan Nedeljković, Academician, a fantastic collection of 20,000 folk poems from war, revolution and socialist building has been gathered and archived (Urošević 1972: 10–11). Some of that material was processed and published in editions of the EI SASA.[22]

In the first years after the war, the development of science was defined as a priority of the new state. Many ambitions could be felt in the plans of the Institute – a large network of collaborators, the creation of questioners for systematic examinations of large areas, planning comprehensive syntheses, the systematic use of archive funds, and so on. The financial possibilities for this in the aftermath of war were however modest. Also, taken broadly, although the number of scientific institutions in the first years after the war significantly increased, it was obvious that the development required better systematization and more rational investment. This was discussed around the end of the 1950s, when society made stronger economic progress. A question was posed not only of investment in development, but also of social relevance of scientific research, what social needs the science would meet. During the course of 1958, in accordance with the new Law on Reorganization of Scientific Work, which was passed at that time (MPE 1968), the goals of the work of the EI SAS were redefined. With an aim to harmonize the work of the Institute with 'our social reality', the Rules of Organization and Operation of the Ethnographical Institute stipulate the following tasks and goals of work:

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[21] Some research was carried out on Kosovo, however the results have not been published, but the finds were archived (Zečević 1952: 580).

[22] For example, two collections edited by D. Nedeljković, *Zbornik radova* [*Collection of Papers*], vol. LXVIII, EI, vol. 3, Belgrade, 1960 and *Zbornik radova*, vol. LXXV, EI, vol. 4, Belgrade, 1962.

(1) Ethnological, anthropological and economic–social study of folk life and customs, folk creativity, settlements and origins of population in past and present;
(2) research of retrograde habits and customs in folk life which slow down its development and attempts to eradicate these habits and harmful residues of the past as soon as possible;
(3) public dissemination of knowledge about cultural achievements of our nations in the country and abroad;
(4) publishing in our own and foreign languages;
(5) taking care of the professional development of young scientists, for successful fulfillment of before mentioned tasks (Drobnjaković 1958: 2).

It can be seen in item (1) that tasks were expanded to socio-economic problems. Item (2) is illustrative by itself for the spirit of time, but according to our knowledge it did not leave a real trace in the work of the Institute. Moreover, the majority of researchers – the ethnologists – kept a personal affinity for topics like customs, tradition or folk religion, although at that time they were still pushed into the background.

The Division of the Institute into five sections remained in force. This division, however, has never been carried out too strictly, due to the small number of researchers and in time, during the 1970s, it became irrelevant for the organization of work in the Institute.

A new and outstanding problem that has gained attention within the Institute ever since then and which will long remain is that of cultural dynamics, i.e. cultural changes under the influence of modernization (industrialization, urbanization). Inclusion of the Institute in social flows, as well as the justification of the social need for ethnologic research, was sought first through the study of the lives of new worker settlements, small industrial towns etc.[23]

In 1961 within the framework of the new five-year plan, project tasks were subordinated to burning problems in the economy of that time – problems of urbanization, i.e. 'rurbanization' of cities, village–town fluctuation and problems of cultural adaptation. A long-lasting research of Djerdap

[23] At that time, scientific institutes were not financed *per* project, but they had fixed long-term tasks and within that and in accordance with five-year state plans, they brought five-year programmes, approved by the authorized Republic Secretariat of Science. Within this framework, researchers were assigned tasks, long-term themes. Topics of PhD dissertations of young researchers were also determined by scientific administrative bodies of institutes. In the early 1960s it was common for such topic to be an anthropogeograhic-ethnographic study of a certain area. From the 1970s the principle of contracting themes for projects with the Republic Secretariat was introduced, and since the end of the 1980s – applications for projects (Drljača, Int).

started in 1962, approved and financed by the Governmental Commission for Djerdap.[24] The project was designed broadly. Beyond a proposal that was submitted to the Government about how to carry out the displacement of the population, a complex registration of the way of life in these villages was also suggested. The reason beyond was that this life-style as well as the Serbian-Vallachian symbiosis specific for this region would be destroyed by the expected changes. The third aspect was almost never reached, as the plan was reduced due to inadequate financing of the project (Drljača, Int.) – a clear sign of the relatively poor position of ethnology in society of that time.

As a comparative example we can take the Institute of Social Sciences. In the opinion of the then researchers of the EI SASA, its financing and possibilities for work were incomparable to those of the Ethnographical Institute. Immediately upon its foundation in 1957,[25] it became the leading research institution in social sciences of its time. The projects, however, were in the function of politics – among other things they were designed to give expert, consultative assistance to the government (D. Drljača, Int.).

In 1966, a new five-year plan was brought in. Beside continuing the examination of Djerdap, the Institute commenced studying national minorities in Vojvodina (Slovaks, Rusyns and Ukrainians) and Serbs in Romania (Urošević 1972: 9).

From 1971 to 1981, the 'contracted long-term themes' with the Republic Community for Scientific Work of SR Serbia were:

(1) permanent follow-up of changes in folk culture in rural areas;
(2) ethnological recognition and examination of endangered places in the field planned for the building of HE power plant Djerdap II, then of the dam on Rasina and on the Belgrade-Bar railway line;
(3) monographic and anthropogeographic–ethnological research of Lužnica and Budžak in Eastern Serbia and Šabac, Posavina and Pocerina (*ibid.*.: 9–10).

By observing the period from the end of the 1950s to the end of the 1970s we can see that, little by little, it became obvious that ethnology in Serbia did not have sufficient capacity to be able to return from the periphery to the centre of those social sciences with better defined goals and higher academic status. The way in which this science was traditionally based here, its theoretical–methodological frames and education, which ethnologists obtained during schooling, did not give stable ground for such a possibility. In comparison with sociology, which in that period rapidly developed, ethnological

[24] This was about problems resulting from building the Djerdap hydro-electric power plant – some places were foreseen for dislocation and submersion in the building of the HE power plant and creation of an artificial lake.

[25] Earlier, the Institute was a two-year School for International Relations.

works left the impression of having an insufficient theoretical foundation and superficial description. During these years, the tendency towards interdisciplinarity, as it was originally created, was lost – sociology distanced itself, folkloristics started to be recognized as a separate science, although it remained in close relations with ethnology–anthropology until today, and towards the end of this period, anthropogeography also distanced itself, returning under the wing of geography. This resulted in a gradual return of ethnology to its own foundations – a gradual return to culture as a basic subject of research.[26]

'Folk life', customs, beliefs, and even the term of *ethnos*, 'rediscovered' in the middle of the1960s – again become a subject of interest, but still with very poor financing,[27] only a small number of researchers[28] and its own feeling of marginalization, which was preserved until today and is frequent in oral folklore within the branch. On the other hand, also, dealing with studies of cultural change remained as a permanent orientation among researchers.

In addition to the strategy of dealing with problems of applied ethnology and cultural changes related to actual processes of social development, which turned out only partially successfully, another strategy for exit from this marginalized position, implemented particularly since the mid 1980s was anthropologization – theoretical–methodological and the problems with harmonization of ethnology in Serbia with the trends of cultural anthropology of the time.[29]

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[26] It is indicative that in the mid 1960s approved themes for Ph.D. dissertation were, for example, Vuk Karadžić as a Folklorist, or Kolo-dance among the Southern Slavs, and not anthropogeographic monographs (Drljača, Int.).

[27] For example, the Bulletin was not published from 1961 to 1969 due to insufficient funding of the Institute (Urošević 1972: 11).

[28] When it was established, the Institute had four full-time employees and their relatively small number remained its characteristic throughout the entire observed period. In 1955, after eight years of work, it had only three scientific associates and seven assistants, and in 1982 – twelve full-time employed ethnologists and thirty external collaborators. The small number of permanent staff was partly compensated by relatively diversified network of external collaborators (Vlahović 1982: 11), which in the beginning was extremely interdisciplinary, including also Montenegro, Macedonia and Vojvodina – besides ethnologists and folklorists, at that time there were also geographers – anthropogeographers, architects, linguists, sociologists, historians, archeologists, even pharmacists, who studied folk medicine. During the 1960s, graduated ethnologists slowly prevailed as employees and external collaborators, or more often students of ethnology, either graduates or postgraduates.

[29] That strategy was brought and implemented by new generation of associates, educated during 1970s and 1980s on the Faculty of Philosophy, as well as individuals like Professor of the Faculty of Philosophy Dušan Bandić, who was also the Director of the Institute in 1984–9, and later, Dr. Ivan Čolović, Associate of the Institute from 1990 to 2003.

Since 1981, all researchers of the Institute have been engaged on a common project – the *Ethnic and Ethnological Characteristics of the Serbian Population*. Also, efforts were made to prepare the *Serbian Ethnological Dictionary*. Signs of change are nevertheless visible: there was no more anthropogeography, urban anthropology is being introduced (sub-project *Ethnological Investigation of Urban Milieus*), symbolic and communication functions of culture are studied, and so on.

Anthropologization of science is obvious, but at the same time there are also compromises and theoretical–methodological disparities: up to the end of the observed period entirely different concepts coexisted in a latent conflict. That conflict took the shape of a conflict of generations – the younger generation perceives the present condition, which is said to be represented by a generation educated in the years immediately before or after World War II as 'our science of us', 'science of people', where allegedly rural origin is considered a condition for the understanding of 'us', but first of all as a science without method or theoretical foundation, and it attempts to do something against it. Urban anthropology, studies of gender, ethnic identity, symbolic and communicational aspects of culture, and first of all structuralism, and studying of transition rite, mark that 'new' as it is understood by present generation of young ethnologists (Kovačević 2005, 2006).

When we look at what happened within the Ethnographic Institute during the observed period (1947–91), there are notable consequences of relative marginalization: desires and set goals were often higher than possible. On several occasions, the set goals, for creation of larger, synthetic works, as well as for a dictionary or encyclopedia of ethnological terms – were not achieved. Publishing is characterized by relatively small numbers of monographs, key works, syntheses – researchers work fragmentarily, and publish their works most often within a *Collection*. More voluminous editions, besides regular publications,[30] are the monographs 'Yugoslav Peoples',[31] several remarkable ethnologic and anthropogeographic ethnographic monographs,[32] a Collection of Papers on Cultural Changes,[33] three Collections on Djerdap,[34] several monographs published at the end of the 1980s,

[30] Bulletin and Collection of Papers of the EI SASA.

[31] This publication is a result of cooperation with the Soviet Academy of Sciences. 'Yugoslav Peoples' were published in 1964 in Moscow, within the book *Narodi Zarubeznoi Evropi*, published by ANSSSR. A significant number of scientists from the whole of Yugoslavia worked on this besides the EI researchers. The following year saw the publishing of the same book in Serbian, as a separate issue of the Ethnographic Institute of the SASA (1965).

[32] For example, Mladenović 1973; Blagojević 1979; Nikolić-Stojančević 1989, etc.

[33] *Symposium Ethnological Study*…. 1974.

[34] Collection… 1973; 1974; 1976.

which are in fact the MA theses of the Institute's researchers,[35] as well as publications of the Department of Social Sciences which represent the work of the Ethnographical Institute.[36]

We can ask ourselves – what is the situation with studying ethnos/ethnic identity in this context in particular?

Ethnic phenomena have started to gain greater attention within the work of the Institute since 1966. Since then, the study of small ethnic communities in Vojvodina (Rusyns, Slovaks, Poles, Ukrainians) has become one of the research goals of the Institute. Studying the Serbian minority in Romania also started at that time, in the village of Svinjica in the Romanian part of Djerdap, on which a monograph was also published (Collection…1971). Apart from this, there was no systematic, complex, long-term research of Serbs outside Serbia.

Around the end of the 1970s, a considerable interest in the problems of emigration, national minorities, new economic migrations emerged in the Ethnographic Institute as well. The first project dealing with emigration was drafted in 1978.[37] However, the first proposal of the Institute to commence research of our emigration was refused by the Republic Community for Science, as the issue of emigration was highly politized and at that time it could be dealt with only by reliable, 'well-tested cadres' i.e. researchers. However this climate gradually changed, Matica of Emigrants was involved as well, and a Board for Scientific Examination of Emigration was founded. Gradually, recognizable support for this project was created by a greater social interest. The Zagreb and Ljubljana Institutes for the study of migration successfully developed their activities, wherefrom the Ethnographical Institute also received some encouragement (Drljača, Int.).[38]

The new operation plan of the Institute, adopted in 1980 (period 1981–6), also included the theme of Emigration from Serbia (within this theme, the emigration of members of national minorities was also studied, for example the emigration of Sandžak Muslims to Turkey). However, the interest for Serbs outside Serbia prevailed over interests in other nations or national minorities in Serbia.

Thus within the possibilities, regardless of the title of the project, it was designed to examine Serbs in the diaspora, emigrants, temporary work-

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[35] Pavlović 1990; Antonijević 1991; Prica 1991; Lukić-Krstanović 1992.

[36] Symposium On Methodology…1974; Antonijević 1971; Nikolić-Stojančević 1974.

[37] Particularly responsible for the introduction of these problems is Dr. Dušan Drljača.

[38] This can point to new trends of republics closing within their borders and reconstructing ethnic–national solidarity within them, as well as to the beginnings of revalorization of recent history, i.e. emigration, which in the period of socialism was determined as 'hostile emigration'.

ers and (our) national minorities in neighbouring countries. Although the authors of the project proposal were aware that the research would not be as voluminous and complex as it should have been –basic tasks included examinations of problems of preserving and losing awareness of national/ethnic belonging, subjective and objective dimensions of these processes, symbolic limits of a group, situational identity, integration, acculturation, preservation of traditions brought from homeland, relation with homeland, relationship of 'we' vs. 'others', influence of new waves of emigrants on the existing ones (*ibid*.). Within that project, during 1984 and 1985 research was carried out in Serbian communities in Chicago (Mirjana Pavlović), Toronto (Miroslava Lukić-Krstanović) and Duluth, Minnesota (Dušan Drljača). The most important results of this research were published in two volumes,[39] and can be summed up first of all as an observation of ethnic identity as a subjective, symbolic process in synchronic and diachronic perspective, and also as an observation of the broader social context/social factors influencing the integration, assimilation or preservation of national identity. With works of previously mentioned E. Petrović, it can be said that between the end of the 1980s and the beginning of the 1990s the Barthian approach to ethnicity as a symbolic process and historical and contextual social phenomenon settled here. It is even more important to note that behind the process of changing the ethnos–ethnic identity paradigm, in fact there is a change of paradigm from *ethnology as a science of people* into *ethnology as a science of culture*. Ethnic identity, therefore, is only one of the aspects of culture taken as a symbolic system; so far, within the Ethnographical Institute, besides this approach to ethnic phenomena, studies of cultural change, communication aspects of culture, and examination of modern popular culture and everyday life also settled and became theoretically founded.

It is assumed that theoretical–methodological competence and scientific relevance are the first necessary steps toward social visibility of a science. In the long run, they should represent fruitful steps in the exit of ethnology from a marginalized social position. However, its marginalization is present even today. It can be assumed that in order to overcome it, the second step is necessary as well, besides its already achieved *scientific relevance* – more self-confident coping with *socially relevant* themes, which in the case of the study of ethnic phenomena would include their political aspects and ethnic conflicts as well. It remains for ethnologists in Serbia to win a change of status, but before that, to decide whether they wish a role in

[39] MA theses of Pavlović 1990; Lukić-Krstanović 1992.

mainstream science at all, i.e. to foresee advantages and disadvantages of such position.

In the context of studying ethnic phenomena, it is interesting to turn also to problems in the field, i.e., methodology of field research in the Institute's projects.

The Ethnographic Institute settled a fieldwork which understood short, often collective[40] several day-long stays in the field, and work with questioners, or even polls ('poll sections').

There is a visible decline in interest for traditionally understood field research in Serbian ethnology, starting in the 1980s – in ethnology/anthropology itself the importance of fieldwork is reconsidered, and interests of new generations of scientists are directed in a way that 'field is everywhere around us'. However, fieldwork remains the main distinction between ethnology and other sciences. In the case of studying the diaspora (Canada, Chicago), the situation was exceptional in relation to what had been common by then – researchers spent at least two months each with communities they wrote about, in emigrant families, both physically and empirically far away from their usual everyday routine. This method of work was suspended in the 1990s – sanctions and impoverishment of society hindered further systematic investment in such research.

Although post-modern critics in fact question this approach to field research ('I know that because I was there'),[41] in the context of ethnology in Serbia it represents a big step forward in comparison to the usual fieldwork.

In the end, bearing in mind the framework topic – ethnology under socialism – we can raise the question of the eventual influences of ideology and government in the selection of researchers of the Institute and their work, i.e. 'social control' of the Institute at the time of the single-party system. The impression is that a certain kind of pressure existed, but that it was relatively low. Membership in the then only party was considered an advantage, but in no way was it a condition for getting a job, and discrimination of non-members was later relatively small as well. Moreover, in the

[40] This means that researchers in the field maintain the usual social relations among themselves which makes them a group opposed to the examined and makes participation more difficult. If it is about rural places – since 1970 the usual accommodation of researchers is in a hotel, out of the place itself, which reduces the observation of the researcher to a small measure. Such field research in fact does not understand the need for participation. Besides a positivist relationship toward 'objective facts' which in this way should just be gathered, it can be assumed that the concept of 'our science of us' lays behind it – mutual understanding of 'researchers' and 'researched' is understood by definition and long-term 'getting accustomed' to the position of the 'other' is in fact not necessary – 'other' in that concept does not exist.

[41] See, for example, Ivanović 2005.

Institute the directors themselves most often were not party members, with only one exception. Researchers of the Institute nevertheless remember *éminences grises* (usually an Academy member and party official in the same time), who had certain power not only based on scientific respect, but also on party position. Their influence was felt, but it was not drastic (Drljača, Int.). That is in fact a logical result of the marginalized position of ethnology in the period of socialism – since it was not important and since it was relatively invisible, it was freer from this kind of pressure.

Conclusion

In this paper I have tried to present the way ethnology in Serbia was practised, while studying ethnic phenomena, through (1) changes of the paradigm itself, and (2) through the work of the institution, which in the time of socialism was assumed as the key for this kind of research. The work in fact shows marginalization of the position of ethnology in the time of socialism as its key problem, and it shows also the attempts within the branch to exit from that position. In the case of the Ethnographical Institute of the SASA, these efforts were carried out in direction of applied research, studies of cultural changes of contemporary society, and then also through the return of ethnology to its own scientific tradition – this time however not by the study of *ethnos*, but of *culture*, and particularly through the process of *anthropologization* of ethnology – its more serious theoretical–methodological foundation. Formulation from *ethnos* to *ethnic identity* pertains to both processes – understanding ethnology no longer as a *science of people*, but as a *science of culture*, as well as its *anthropologization*. In spite of this obvious progress and obtaining scientific relevance and foundation of research and results, however, social marginalization has to this day remained the biggest subjective and objective problem of ethnology in Serbia, which yet needs to be resolved.

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Chapter 13
Displaying National Culture in Antinationalist Times: The Ethnographic Museum in Belgrade under Socialism

Marina Cvetković

The aim of this paper is to discuss the main features of the functioning of the Ethnographic museum in Belgrade during the period of socialism (1945–90). Due to the complexity of the institution whose official tasks were, and still are 'to collect, preserve, study, interpret and display' Serbian and, during certain periods, Yugoslav, ethnographic cultural heritage, the paper will focus primarily on exhibitions and scientific research activities. Analysis is based on publications, exhibition catalogues and archive sources of the Ethnographic Museum, as well as information orally conveyed from the museum curators who were active during the era of socialism.

To begin, we consider it necessary to turn briefly to the period of establishment of this institution, to the period when basic guidelines for its further functioning and development were laid down.

In contrast to Western European and American anthropology, both of which came into being during the colonial period, and thus put the culture of 'Others' at the centre of their research, Serbian ethnology, which became institutionalized in the period of state and nation building, turned to the study of its own 'native' culture, and in particular to rural culture as an 'embodiment of national identity', much like analogous disciplines in other Central and Eastern European countries.[1] This approach defined the standards for the collection and presentation of objects in ethnographic museums, which in fact meant that attention would exclusively be given to artefacts of rural culture produced in the pre-industrial era. Such a conception was adopted in

[1] Such concepts were inherited from the romanticist movement, and in particular from the 'father of Serbian ethnology' Vuk Karadžić who was acquainted with the ideas of Herder. During the lifetime of Karadžić, most Serbs lived in villages and that was the reason why the concept of rural and folk were merged. This timeless, static idea of the subject of ethnology was uncritically conserved in Serbian ethnology, regardless of numerous social, historical economic changes.

the Ethnographic museum in Belgrade, like in many other similar institutions, and remained dominant during most of the twentieth century.

In the period of state building, territorial expansion and national unification, folk art became one of the privileged means for the expressing and upholding of the nation's identity, and for the legitimization of nationalist political projects. The young Serbian state stimulated research on various forms of folk art and supported the organized display of such objects in exhibitions at home and abroad in order to give credibility to its political goals and to uphold its international prestige.

Of central importance in the establishment of the ethnographic Museum in Belgrade was the All-Slavic Exhibition in Moscow, held in 1868 as a realization of pan-Slavic ideas (Milićević 1885). For this occasion, items of Serbian rural culture from Serbia and parts of Austria-Hungary populated by Serbs were for the first time displayed together in the same place.[2] The cultural and political implications of such an event motivated political figures and scholars in Serbia to consider the establishment of an Ethnographic Museum in Belgrade. Thus, the prominent legal scholar Valtazar Bogišić reflected about a museum which would materialize the ideas of pan-Slavic unity, whereas in 1872 the historian, politician and diplomat Stojan Novaković presented in the Serbian Ethnographic Journal his first draft of the future Serbian Ethnographic Museum. In his opinion, the Ethnographic Museum should represent 'a mirror which clearly reflects the cultural history of the nation' (Novaković 1872: 337). He stated that it was 'high time to found a historical-ethnographic museum, in which our community could be observed in its overall diversity through monuments of our ancient culture, to serve there as a silent and at the same time powerfully eloquent lesson about the unity and hallmark of our nation' (*ibid.*: 347). Nearly three decades passed before the conditions for the establishment of such an institution occurred (see also Knežević 1971: 17–27). The Ethnographic Museum was officially opened in 1901 by separating the ethnographic collection from the National Museum. Thus was created the first ethnographic institution in the country.

In the initial years, the Museum had to devote itself to fervent collecting activities, and in 1904 it was ready to open its first permanent exhibition, on the occasion of a very important anniversary – the centenary of the First Serbian Uprising. The first permanent exhibition clearly represented the materialization of the conception put forward by Stojan Novaković. It displayed object collections gathered among Serbian people on the territory of the Kingdom of Serbia, but also Austria-Hungary (Dalmatia, Slavonia,

[2] Through a network of collaborates, the material had been gathered on the territory of Serbia and in some parts of Austria-Hungary. See Karpova 2005.

Bosnia) and the Ottoman Empire (Kosovo and Macedonia), thus materializing and legitimizing Serbian territorial aspirations. The exhibition was clearly considered as an event of greatest national importance. It was ceremonially opened by the recently crowned King Petar I Karadjordjević,[3] in the presence of all representatives of the Serbian government.

Up to the liberation wars 1912–18, the Ethnographic Museum was intensively collecting objects and participated in prestigious European exhibitions with a clearly defined national mission.[4] The exhibited items were to affirm the authenticity of the Serbian nation, but also to legitimize the right of all Serbs to live in a common state.

After a turbulent period of war, 1919 saw the establishment of the Kingdom of Serbs, Croats and Slovenes. The direction of the official policy shifted from Serbian nationalism to Yugoslavism. In this new frame, the Museum's activities were expected to point to similarities and the common origin of the 'three tribes' of Serbs, Croats and Slovenes. New Ethnographic Museums were opened in 1919 in Zagreb, and two years later in Ljubljana.[5] On the territory of today's Macedonia, which was then called Southern Serbia, intensive field research was carried out, as well as the acquisition of materials with Serbian national attributes. Between the two wars, the activities of the Ethnographic Museum were focused on the systematic collection of materials from the territory of the kingdom of Serbs, Croats and Slovenes, and after 1929 the kingdom of Yugoslavia. However, objects were in fact mainly collected in Serbia, Macedonia, Montenegro, Bosnia and Herzegovina and those parts of Croatia where Serbs were a majority. The institutionalization of ethnographic museums in other national centres, together with the ethnically and nationally determined policy of object collection, in practice meant that the museological application of the idea of Yugoslav national integration was moribund from the very beginning.

In this period, exhibition activity was not as intensive as in the previous one, but much effort was put into organizing the publishing activity of the Museum. Thus, the new permanent exhibition inaugurated in 1924 was accompanied for the first time by an exhibition guidebook (Zega 1924). In 1926, the Museum started to publish the *Bulletin of the Ethnographic Mu-*

[3] In 1904, after a military coup that deposed the Obrenović dynasty, the rivalling Karadjordjević dynasty took back the throne in Serbia.

[4] The most important ones being: 1902, Exhibition of Costumes in Petrograd; 1904, Permanent Exhibition in Belgrade; 1905, Industrial Exhibition in Liege; 1906, Jubilee Exhibition in Bucharest; 1906, Journalistic Exhibition in Paris; 1907, Balkan Exhibition in London – the biggest exhibition of the EM; 1911, International Exhibition in Turin; 1912, All-Slavic Exhibition in Prague.

[5] In 1921, Niko Županić left the Ethnographic Museum, having been appointed the manager of the Ethnographic Museum in Ljubljana. See Drobnjaković 1926: 22.

seum, which up to nowadays represents a 'mirror of research activities of the Museum'. In the Introduction to the first edition of this specialized journal, it was said that researchers should 'raise questions that will contribute to gaining deeper and more detailed knowledge of our nation'.[6] The first fifteen volumes of the *Bulletin* were published in the period from 1926 to 1940. These volumes contain numerous ethnological studies and discussions, articles and reports on the work of the Ethnographic Museum by the leading scholars of the discipline.

From 1945 to 1960

The main legacy of pre-war conceptions and activities of the Museum consisted in the following:

- (a) an almost exclusive focus on objects of pre-industrial rural material culture of the Serbs;
- (b) pronounced political mission in the frame of which the public exhibiting of 'national culture' was seen as a strategy of legitimization of national(ist) aspirations, and finally,
- (c) mixed results, if not outright failure of post World War I efforts to produce and display an integrated supra-national 'Yugoslav culture'. Representatives of the new socialist regime tried to come to terms with this legacy, but their results, as we shall see, were mixed at best.

The end of World War II and the success of Tito's communists in the short ensuing power struggle brought changes of existing social, economic and territorial structures. On 29 November 1945, the Federal Republic of Yugoslavia was proclaimed; while in January 1946 the first Constitution of the new republic was passed, defining the new socialist social-economic system and new federal system incorporating six republics. Macedonian and Montenegrin nations gained 'equal status' with other nations of the Federation, and each 'constitutive' nation was nominally accorded a republic of its own, with Bosnia and Herzegovina remaining the only odd man out until 1971, when Muslims were accorded the status of nation. The new approach to the 'solving of the national question' was marked, among other things, by the opening the Ethnographic Museum in Cetinje (Montenegro), and by the reorganization of the former Museum of Southern Serbia, renamed in 1949 as the Ethnological Museum in Skopje.

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[6] Uprava Etnografskog muzeja u Beogradu, (uvod), Glasnik Etnografskog muzeja u Beogradu /*Head Office of the Ethnographic Museum in Belgrade, Introduction, Bulletin of the Ethnographic Museum in Belgrade/,* vol. 1, Beograd, 1926.

Interestingly enough, all existing ethnographic museums were once again limited to studies of 'nations and nationalities' living within the territorial borders of their respective republics. Although in the beginning it was intended that permanent exhibitions of the Ethnographic Museum and the spectacular visiting shows should represent folk, i.e. the rural culture, of socialist Yugoslavia as a whole, most of the research and collecting activities by the personnel of the existing ethnographic museums in the post-war period were carried out on the territories of their nominal republics.[7]

The period from the end of the war to 1951 in the Ethnographic Museum can be characterized as a time of reconsolidation and reconsideration, and from 1951 to 1960 as a period of growing stability.

In 1945, the Ethnographic Museum was moved to the repaired central arched building of the former Serbian Royal Court. The former residency of Serbian monarchs thus became a building dedicated by the new communist authorities to the 'Working People and Honest Intelligentsia'. A year later, a new Permanent Exhibition was symbolically opened in these premises, encompassing folk creativity of Yugoslavia, thus legitimizing the new territorial arrangements and the new course of national policy.

In an uncertain aftermath of war, when many institutions faced closure, the Ethnographic Museum was not only staffed by new employees,[8] but also prepared very ambitious reorganization plans. In 1946, the Government approved the plan according to which the Museum should become a large centre of scientific research with six departments and twenty one sections.[9] Even today, the dilemma concerning the true intention behind this reform still remains. Namely, it is not yet clear whether, following the example of many countries of the Eastern block, the true intention was not in fact to disestablish the Department of Ethnology at the Faculty of Philosophy and reduce ethnology to an auxiliary science of history, specialized in the study of material culture. Notwithstanding its original intentions, this reorganization never came into being. Instead, all the Museum departments that had been active before the war continued to operate: (a) museum collections, (b) an illustrative department, (c) a department for musical folklore, and (d) a library.

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[7] Items displayed in exhibitions presenting the culture of nations and ethnic groups of Yugoslavia were acquired from pre-war funds of the Ethnographic Museum, or obtained through inter-museum cooperation, not by new field projects.

[8] Up to 1950 as many as thirteen curators (or curator assistants) had been admitted. Extract from archive document of the Ethnographic Museum in Belgrade, Službenici Etnografskog muzeja od 1901 do 1906 */Employees of the Ethnographic Museum from 1901 to 1906/*, Glasnik Etnografskog muzeja u Beogradu 39, 1976: 42.

[9] The plan was approved by the Ministry of Education, Science and Culture of the National Republic of Serbia on 23 May 1946. For more details, see Vlahović 1953: 21, 22.

The aftermath of war saw numerous repressive actions such as the confiscation of property belonging to persons 'who collaborated with the enemy'. Furthermore, in 1946 private enterprises were nationalized together with main economic branches, and in 1948 what was left of private property was also nationalized. In the same period, a number of museums and associations were also closed down, and their funds were nationalized and redistributed to other institutions. In this way, the Ethnographic Museum acquired funds of the Belgrade Women Society (established in 1878) and the Circle of Serbian Sisters (established in 1906), which in the pre-war years, among other things, had studied, collected and exhibited folk textile creations and applied their knowledge in curricula of women schools.[10] The Museum also enriched its funds with items from the disestablished Museums of Forestry and Agriculture (Dušković 2001: 42). However, the period also saw an outflow of objects from the funds of the Ethnographic Museum. Military flags and other symbols of national and military history were delivered to newly authorized institutions. The transferred items included 'personal belongings of Prince Mihailo Obrenović, among which also the clothes he was wearing in the moment of his assassination' (*ibid.*). A valuable collection of phonographic records of folk music was delivered to the Institute of Musicology of the Serbian Academy of Sciences and Arts, in the 1960s (*ibid.*).

Nevertheless, in spite of intensive social changes, it can be said that there were no radical cuts in the functioning of the Ethnographic Museum. Mitigation and amortization of big changes was probably related to the fact that the pre-war director of the Ethnographic Museum, the prominent ethnologist Dr. Borivoje Drobnjaković (Director 1925–50) was kept in his post even after the Revolution. In 1950 he moved to the position of Professor of Ethnology (visiting) at the Faculty of Philosophy in Belgrade. A rich experience in museology, and knowledge of European museums and museum issues, enabled him to be the first at the faculty to introduce a course in Ethnographic Museology, which turned to be very useful for the education of future museum personnel (Jovanović 1977: 76; and see Barjaktarević 1961: 275–80). Together with the curators of the Ethnographic Museum, Milenko Filipović, Persida Tomić, Petar Ž. Petrović and others, he published articles and debates from the field of museum issues in the journal Muzeji /*Museums*/ – specialized for museological and conservationist issues, published alternatively in Belgrade and Zagreb from 1948 to 1965.

A novel and important role of scientific and cultural institutions in the early post-war period was their 'participation in social, economic and politi-

[10] Unfortunately, this inventory is incomplete, with few documents identifying it.

cal development of the new socialist society'. The Ethnographic Museum also had an obligation to participate in 'socialist building of the homeland' in a specific way.[11] Museum funds were opening up towards students, professionals, architects, designers. Decorative motifs, shapes and other fine art characteristics of certain items found practical applications in architecture, textile industry, folk art and craft souvenirs. The Museum rendered its services to folklore ensembles, film companies (Avala and Zvezda), national theatres and many other institutions (Vlahović 1953: 21). The idea of treating elements of folk culture as practically applicable patterns and even as consumer goods in a rapidly modernizing society could have sparked off interesting developments in museological research and applied ethnology, but such initiatives withered away during the crisis of the late seventies and early eighties.

The moving of the Museum to the building where it is situated today as well – the building of the former Stock Exchange on Studentski Trg marked a new period in its work. The Museum was symbolically opened to the public on 29 November 1951, on the tenth anniversary of the start of the National Revolution and a half century of work of the Ethnographic Museum. Mitar Vlahović was appointed Director.[12] On the occasion of this anniversary, after a thirteen-year long interruption of publishing activity, a *Collection of Papers of the Ethnographic Museum 1901–1951* was offered to the public. One article drew particular attention. Špiro Kulišić, one of the leading representatives of Marxist ideological currents and a fervent disciple of historical materialism, in decidedly Soviet-style rhetoric attacked pre-war Serbian ethnology, qualifying it as 'reactionary' and 'bourgeois'. He particularly criticized the activities of the Museum in the period between the two wars, during which the 'development of our ethnographic science was conditioned by the policy of national oppression carried out by the governing bourgeois circles. The governing clique wanted to negate ethnic and cultural specificities, national individualities of our nations, denying in that way the right of subjugated nations to their free development. The theory of a unique Yugoslav nation with three tribes, intended to justify the centralistic and

[11] The same task was set before the Slovene Ethnographic Museum, see Rogelj Skafar 2003: 52.

[12] Although Professors Jovan Erdeljanović and Tihomir Djordjević chose him for full time Assistant at the Department of Ethnology, Mitar Vlahović, at his own wish, in 1927 moved to work as a curator in the EM. In the pre-war period, together with Borivoje Drobnjaković, he put great efforts into the identification and systematization of museum collections. During World War II, he stayed at all times with the artefacts, packing, repacking and moving them, thus managing to a certain extent to protect them from being destroyed. See Barjaktarević 1977.

hegemonic policy of the governing clique, reflected itself in our ethnographic science' (Kulišić 1953).

Such ideological disqualifications seemed to fit in particularly well with the official policy that considered 'hegemonism' and 'integrism' as the main threats to the socialist 'fair solution of the national question in the new Yugoslavia'. Even after two decades, similar theses were still advocated by party affiliated scholars such as, for example, Nikola Pantelić. In his analysis of Museum policies concerning the collecting and studying of objects in the period between the two wars, Pantelić concluded that the policies were in a 'close relation with tendencies of Serbian bourgeois class and its hegemonic aspirations' (Pantelić 1976: 58). Špiro Kulisić's answer to the 'malaise' that he so fervently criticized was somewhat confusing: 'the task of the Ethnographic Museum lies in collecting ethnographic objects aimed at the presentation of the past of our nations and the emphasizing of the specificities of their culture' (Kulišić 1953). Rather paradoxically, Kulišić's solution unintentionally supported both the 'antiquarian' conception of ethnographic museums (keepers of objects as relics of the past) and their nation-building role (now supposedly working for the 'oppressed', instead of the 'hegemonic' nation).

In 1954 the publication of the *Bulletin of the Ethnographic Museum in Belgrade* was renewed. Contrary to what might be expected, a comparison of contents of pre-war and post-war issues of the *Bulletin* gives an impression of continuity in scientific and research work. Both pre-war and post-war papers have been divided in similar thematic units: (1) economy; (2) folk architecture and household items; (3) folk costumes, jewellery, textiles, embroidery and lace; (4) customs (general, rite, social, legal; customs related to working; (5) religion, mythology, beliefs; (6) folk literature, music, dance, narration; (7) folk knowledge; (8) universities, museums, relating institutions and societies; (9) biographies and bibliographies and (10) miscellaneous. What is new in post-war editions is the study of: (1) regions and settlements (contributions of various funds); (2) ethnic processes, migrations and population; (3) social life; (4) theories and methods; and (5) museology and protection of museum holdings.[13] In this period, four albums with fine arts material were published.[14] Research organized by the Ethnographic Museum

[13] Compare Drobnjaković 1935: 129–60; and Bosković-Matić 1976: 19–39.

[14] Akvareli Nikole Arsenovića /*Watercolors of Nikola Arsenović*/, Belgrade 1954; Odeća i ornament u Srbiji /*Clothes and Ornament in Serbia/,* Belgrade 1956; Srpski seljački nadgrobni spomenici */Rural Tombstones Monuments in Serbia/,* Belgrade 1956; Narodne nošnje u XIX veku u Beogradu – akvareli Karola Pop de Satmarija */Folks Costumes in 19th Century in Belgrade - Watercolors of Karol Pop de Satmari*/, Belgrade 1958.

up to 1960s was mainly monothematic, and it was accompanied by the deliberate acquisition of new items.

Exhibition activity in the post-war period showed a tendency towards constant growth. Besides permanent exhibitions, occasional thematic exhibitions of a study character were realized after the war. The author of the concept of the first monographic exhibition organized in the framework of the Ethnographic Museum – 'Vojvodina in the Nineteenth Century' - was Dr. M. Filipović. In the same year, 1946, on the occasion of Vuk Karadžić's jubilee, another monographic exhibition entitled 'Vuk's Homeland' (author of the concept Dr. B. Drobnjaković).[15] The first study exhibition that placed the Museum's items in the focus of research was the Exhibition of Kilim-making in Serbia, by curator Jerina Šobić (1950). This exhibition marked the beginning of a long-term practice of exhibiting the Museum's object collections as the basic mode of exhibition activity.

From a political standpoint, the year 1948 marked a breaking point in the relations between Tito's Yugoslavia and the Soviet Union, and introduced new orientations in Yugoslavia's foreign policy. Tectonic changes in the political sphere found their reflections in other domains as well. For example, exhibition activity in the Museum witnessed a warming of relations between the West and the Federal Peoples Republic of Yugoslavia. In stark contrast to the position of ethnographic museums in the countries of the Eastern block, for which in that period the so-called 'iron curtain' implied a break of communication with the West, the Ethnographic Museum could take advantage of freshly opened state borders. That very year, the representative exhibition 'Folk Art of Yugoslavia' was organized in the Art Pavilion on Kalemegdan, wherefrom it started its lengthy journey around Western Europe. In the period from 1948 to 1952 it was on tour in Germany, England, Belgium, Netherlands, Switzerland, France and Norway. The concept of the exhibition was created by distinguished ethnologists-museologists, representatives of Ethnographic Museums of all six Yugoslav republics: Dr. B. Drobnjaković, P. Tomić, M. Gušić, V. Kličkova, M. Gavaci, A. Koludrović, B. Orelj, Z. Čulic-Marković (Lazarević 1976: 33-46). The Introduction to the catalogue prepared for the exhibition in London states: 'The nations of Yugoslavia build socialism in their liberated homeland; and one of their tasks in building socialism is to preserve their cultural heritage',[16] thus determining the role of museum 'in the new socialist society'. Interestingly

[15] Exhibitions with fine art items were also displayed: 1949, Painters Ethnographers Satmari, Arsenović, Titelbah – author of the concept B. Radović; 1950, Old Belgrade in Pictures; 1950, Serbian Rug Manufacturing: J. Šobić.

[16] Exhibition Catalogue, Yugoslav Folk Art Exhibition, London, 1950, III.

enough, folklore was chosen to display the new face of Yugoslav socialism to the West.

Since 1951, curators have started with design and realization of temporary thematic and study exhibitions. For the first time they presented themes such as Folk Music Instruments, Children Toys, Folk Engraving in Yugoslavia. The exhibition entitled 'Rural Tombstone Monuments', created by Mitar Vlahović, became a veritable cultural event in spite, or perhaps because, of its sensitive relation with religion. Exhibitions were also displayed in complex museums in the Serbian countryside (Čačak, Kraljevo), as well as in towns of Macedonia, in Dubrovnik in Croatia and Ljubljana in Slovenia (*ibid.*.).

The exhibition on 'Superstition and Quackery', by Persida Tomić was in many ways symptomatic for the breakthroughs that were achieved during the period. Namely, it had an emphasized educative and critical role, presenting elements of traditional culture as obstacles on the road to full human emancipation in socialism. The exhibition was presented all around Serbia: in Zaječar, Boljevac, Pirot, Niš, Leskovac, and Valjevo, as well as in Skopje. Engaged exhibitions like this one, notwithstanding their often crude and naïve rhetoric, were telling witnesses of the search for new social roles for museums and ethnology in general.

From 1960 to 1976

In the history of Yugoslav socialism, this period is characterized by relative stability, modest growth of living standards, liberalization of political life and strengthening of self-management. Gradually introduced in 1950s as a reform of the political system, self-management soon became the specificity of Yugoslav socialism. In the Ethnographic Museum, as well as in other institutions, the newly introduced Workers' Councils started making decisions on many relevant issues. However, the official policy of the Museum was determined and carried out by the Professional Assembly, which consisted of curators and preparators (with university-level education). At the meetings of the Workers' Council, the Professional Assembly submitted expert proposals and initiatives, which were then voted by all employees. In most cases these proposals were accepted, which shows that curators maintained a degree of authority and influence. The period from 1960 to 1976 in the Ethnographic Museum can be seen as a period of modernization and steady development of museum work. The scope of the Museum's activities was expanded by new departments, and team-based monographic research was introduced besides the usual individual, monothematic approach.

From this period onwards, directors were no longer appointed from among the curators of the Ethnographic Museum. For the first time, to the

head of the house came an ethnologist who was also a prominent member of the Communist Party, the already mentioned Špiro Kulišić.[17] Kulišić was the most outstanding advocate of historical materialism in Serbian ethnology, and a person who was 'aggressive in his demands and intolerant of differing opinions' (Prošić-Dvornić 2003: 387). However, it seems that he came to the Museum when his political power was already in decline. After the liberation he was Head of the Ministry of Education of Bosnia and Herzegovina in Sarajevo, and he gained experience in museum work while working as the Director of the Zemaljski Muzej. From the position of director he had to move down a step, to the position of Senior Scientific Associate and Head of the Ethnographic Department. In 1960 he became Director of the Ethnographic Museum in Belgrade, wherefrom he had to retire in 1963.[18]

Besides his previously mentioned criticism of pre-war ethnology and museum practice, in several ethnological publications he offered fervent ideological attacks on Western anthropology in general and on the functionalist school in particular (Kulišić 1961: 5–24; 1967a; 1967b). Although he did not have many direct followers, it is possible to assume that his extreme attitudes, because they were believed to express the 'official line', silently influenced many ethnologists, particularly curators, to remain cautiously neutral, and not to accept new theories and methods, particularly those coming from the West. While the majority of curators collected artefacts with passion and diligence, processed them in a methodical manner and displayed them skilfully in exhibitions,[19] their shying away from theoretical reflection created a long-lasting 'theoretical vacuum' and established the 'descriptive method' as the prevailing approach.[20] Mirjana Prošić-Dvornić

[17] Here we shall point out the main moments of his biography showing that he had all desirable attributes for a leader in the post-war period. He was born in 1908 in Perast, Montenegro, and in 1933 he graduated at the Department of Ethnology of the Faculty of Philosophy in Belgrade. Before the war he worked as a gymnasium professor in Glina, Prijedor, Čačak, Tuzla. He participated in the War of National Liberation. He was decorated by the Order of Brotherhood and Unity and Order of Merit to the Nation – Direktori Etnografskog muzeja – Špiro Kulišić /*Directors of the Ethnographic Museum/,* GEM 41, Belgrade 1977: 81.

[18] Minutes of the EM show that the Museum initiated his replacement. Officially, the reason was his 'intolerance and arrogance toward curators', however it seems that there were other, party-related reasons as well.

[19] Ethnography – descriptive discipline became hegemonic and suitable for intellectual structure of state socialism, see Reviewed Work(s): Matti Bunz, Fieldsworks and footnotes: Studies in the History of European Anthropology by Han F. Vermeulen; Arturo Alvarez Roldan, American History, New Series, Vo. 98, No. 3 (Sep. 1996): 684–6.

[20] It is interesting that in the mentioned theoretical vacuum only the theory of cultural-geographical areas was highly valued among curators of the Ethnographic Museums in Belgrade, Zagreb, Sarajevo. This theory was the basis of majority of concepts of permanent exhibitions.

suggests that 'the greatest number of Serbian ethnologists (although not folklorists) chose not to be 'politically correct' but rather 'politically invisible', and continued on the margins of scientific arenas to diligently collect material … filling in 'white spots' on maps of researched areas, and saving the remaining traces of traditional rural culture which was quickly vanishing under strokes of new, socialist modernization of the time' (Prošić-Dvornić 2003: 388). Paradoxically, the phase of routinization of communist power seems to have had more stiffening effects than the fervours of its earlier charismatic days.

However, those stiffening effects were relatively slow in pace, and never overtook all aspects of work. Thus, contrary to what might be assumed from his pronouncedly ideological criticism of local ethnography and of Western anthropology, Špiro Kulišić, while being at the helm of the institution, also managed to modernize the work of the Museum, at least to a certain extent. He carried out a broad, comprehensive analysis of activities conducted so far, and in 1960 performed a reorganization of research activities. Curators had to specialize in certain collections and topics, and team research of selected ethno-geographic or politically administrative units was introduced. In the frame of a long-term research plan, the Museum was directed towards the region of Eastern Serbia, and for the first phase its north-eastern part, which is the region of intense Serbian-Vallachian contacts.[21] Besides the research of folk culture in north-eastern Serbia, the task of examining the symbiosis between Serbian and Vallachian population had also been foreseen (*ibid.*.).

The emphasis in monographic research in general was on topics from the fields of economy, social relations, customs and beliefs, and minority cultures. Topics related to textile crafts, folk costumes and rural architecture prevailed in volumes devoted to material culture. In these monographs, museum researchers have for the first time approached their topics in a decidedly anthropological manner. Contemporary influences on changes in traditional culture, the logic of its transformation or articulation into new social and economic structures rapidly gained legitimacy as research topics (Halpern and Hammel 1969: 17-26). Results of first monographic research were published in the *Bulletin of the Ethnographic Museum*: *Gornja Resava*

[21] These researches are expected to give results that will provide a more complete understanding of certain relations and processes from the Middle Ages until the most recent times. All that would together make a significant contribution for the ethnic history of Serbs and, in a broader sense, Southern Slavs as well as other nations and ethnic groups on the Balkan Peninsula. Finally, the economy, social life and spiritual culture of people living in this area have many similarities with, some even share common, cultures of rural societies in all Carpatian countries, which adds an even broader importance to this research. See Pantelić 1966: 9.

1962. Two years later, results of research on Jadar – Vuk Karadžić's region of origin were published. This tradition of research also continued during the terms of office of later directors, with the publication of: *Ethnological Research in Donja Resava* (1966), *Negotinska Krajina* (1969), *Vicinity of Bor* (1975), *Zaječar and its Vicinity* (1978), *Ethnological Research in Titovo Užice, Požega and Kosjerić Region* (1984), and *Sjenica-Pešter Plateau* (1989, 1992). The Ethnological Museum also coordinated research in Serbia for the *Ethnological Atlas* – a large project initiated in 1962 by the Ethnological Society of Yugoslavia, one of the very few collective endeavours uniting ethnologists from all Yugoslav republics (see Bjeladinović-Jergić 2001: 36).

During this period, in 1960, the Documentation Department was established, with the basic task to research, collect, process and maintain documentary material of an ethnological character (Ćertić 1977: 59). For the purposes of rationalization of data searching and improvement of quality of information on the Museum holdings, this department, which employed a curator, a technician and a photographer, created a new documentation system which is in use even today (Reljić 1968: 130–53).

That same year the EM established a Department for Educational and Pedagogical Activities as well. The initial operation plan for this unit stated that its task should be 'planned work in order to attract museum audiences, in the first place school youth, military personnel and workers, and also to bring exhibitions closer to the public as much as possible by audio-visual means'.[22] The unit was oriented toward working with Museum audiences through exhibitions, lectures, concerts and workshops, as well as toward work with the media and various external collaborators (Marković 1971: 28). It is evident that the number of exhibitions has been permanently growing since the formation of this department. Exhibitions escaped the framework of the Museum and became profiled according to their basic intention. Besides large permanent exhibitions and occasional study or thematic exhibitions, appropriate and movable thematic exhibitions have been introduced, with an aim to bring the rich and diversified Museum holdings 'closer to different layers of the socialist society' (Lazarević 1977: 45–9). An example of this was the exhibition 'Belgrade Vicinity Once Ago and Now' displayed in 1962 with the aim to visually complement curricula for the school subjects 'Knowledge of Nature and Society' and 'My Homeland'. During the course of a school year, this exhibition visited nine Belgrade schools and was also displayed in the 'City of Pioneers' children centre, in two cultural

[22] Establishment of this department cannot be attributed only to the initiative of Špiro Kulišić. The initiative partly came from the Coordination Board of Belgrade Museums.

centres and in the building of the Vračar Municipality. It was seen by about 50,000 visitors.

From 1966 to 1977 the Director of the Ethnographic Museum was Slobodan Zečević.[23] He continued work in reforming the Museum and contributed to the development of certain museum activities. The long-term experience and engagement of Dr. Zečević in the creation of cultural policies for the city, together with the energy of curators-pedagogues, contributed to the intensification of exhibition activity.[24] The frequency and thematic diversification of exhibitions in the Ethnographic Museum contributed to the dynamism of cultural events in the city and in the republic as a whole. Exhibitions were removed from the framework of the EM and entered new cultural institutions established in the socialist period on the territory of Belgrade, Serbia, and other republics. Appropriate exhibitions were displayed in the Youth Centre in Belgrade, in the Belgrade Fair (Fashion Fair, Furniture Fair, etc.), in the Belgrade Drama Theatre, in the recently opened Sports Hall in New Belgrade, on the 'Golden Hands' exposition organized by Radio Belgrade, as well as for the live broadcast show 'Selo veselo'. In the countryside, the Museum was a guest on appropriate folklore-type exhibitions, evaluating and affirming various modes of folk creativity (Lazarević 1981: 39–54). According to the opinion of Slobodan Zečević, the artistic characteristics of Museum items exhibited in factory halls, textile industry plants and trade fairs should have served as an inspiration to designers, for the reshaping and redesigning of articles of mass production. In that way the Ethnographic Museum should have 'contributed to the creation of a national industrial design pattern' (Zečević 1971: 12, 13).

From 1966 onwards, the EM became engaged in a new cycle of external activities, and visited socialist Czechoslovakia, as well as towns in Western Europe like Vienna (Austria), Shoten (Belgium), Glasgow (Great Britain), Antwerp, Liege (Belgium), Braunschweig, Hanover (FR Germany), and also Bogotá (Colombia). The Ethnographic Museum also hosted foreign exhibitions, thus presenting to the local public the folk arts of Chile, Mexico, Norway, Romania, Oceania (UNESCO), as well as the Life of the Canadian Eskimos etc. (Lazarević 1981: 39–46).

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[23] Zečević studied law and ethnology. He graduated in law in England, and also spent some time in France and Germany where he learnt foreign languages. He obtained his Ph.D. at the Department of Ethnology in Belgrade, in 1962. Since his early youth he participated in amateur cultural-artistic activities and was a member, secretary, and art director of many amateur cultural-artistic societies and associations.

[24] He was a member of Cultural-Educational Chamber of the Assembly of the City of Belgrade, of the Council of Economy Planning and Finance, of the Board for Culture of the same Assembly, etc. and twice a recipient of the Medallion of the City of Belgrade, *ibid*.

Furthermore, since 1965 the exhibition catalogues were gradually transformed from supplementary informative materials to serious museological monographs on the exhibited collections, i.e. themes.

From 1978 to 1990

In 1974, a new Constitution of the Socialist Federal Republic of Yugoslavia was adopted, vesting broader governing rights in the republics and autonomous provinces (Vojvodina and Kosovo and Metohija, both in Serbia). The Constitution brought about the quasi-confederalization of Yugoslavia. Although earlier research by the ethnographic museums was also carried out mainly within their own territories, republic borders now became an almost insurmountable obstacle.

From 1978 onwards, directors of the Ethnographic Museum ceased to come from the ranks of ethnologists. Rather, they were 'functionaries delegated to the function' from among the ranks of various 'social-political structures' (Bjeladinović-Jergić 2001: 39). The politically appointed directors gradually distanced themselves from the basic museum activities. The worsening political and economic crisis could not but reflect itself onto the Museum work, and a slow decadency in most of the museum activities could be observed.

However, the decline was anyything but mechanical. In 1978 Gvozden Jovanić, a lawyer with a long working experience in 'socio-political organizations', was appointed as Director. Before, he was an Assistant Minister for Culture in the Government of Serbia and one of the founders of the Vuk's Endowment and the Endowment of Ivo Andrić. Despite not belonging to the profession, he proved to be a capable 'cultural manager'. Some curators still call the period of his office the 'golden age of the Museum'. He was most remarkable for his extreme commitment to solving problems of lack of space for storage of collections and exhibition of artefacts, which had burdened the Ethnographic Museum ever since it was established. During his office, exhibition activities were still carried out with the same intensity as in the previous period. The Museum continued to exhibit in cultural centres, in the City of Pioneers children centre, on fairs, on the 'Winter Magic' trade fair, in modern furniture showrooms (Novi dom), factory halls, hotel lobbies and the luxurious interior of the Intercontinental Hotel, and in the newly-built 'Sava Centre'. With prestigious exhibitions it visited London, Leningrad, Istanbul, Moscow, Creusot (France), Sofia, Budapest. The exhibition entitled 'Socialist Yugoslavia' even reached far away countries such as Australia, PR China and DR Vietnam (Lazarević 1981; Mitrović 2002; and Zec 2002: 319–33). Paradoxically, Yugoslav

leaders found nothing more appropriate to celebrate the 'achievements of socialist development' than 'the most beautiful pieces of folk art'.

After this period of prolonged success, in 1986 a new director was appointed – Branko Radivojević, a professional politician who had graduated at the Advanced School of Special Education. Earlier he had been engaged in the city social structures (Deputy Mayor of Belgrade and President of the Zemun Municipality). His appointment coincided with the suspension of the Professional Assembly, which, in earlier times, as an expert body of ethnologists and conservators, had an important and often decisive role in the devising and supervision of work schedules (Bjeladinović-Jergić 2001: 39). The estrangement of the directors from museum experts and their ignorance concerning problems and needs of professional work gradually led to the strengthening of the General Services unit, i.e. museum administration, and to an ever-deeper gap between the experts and the administration. The social crisis, rise of nationalism and turmoil characteristic of the eighties could not but mark the work of the Ethnographic Museum. Certain politically and nationally sensitive curators harboured the feeling that previously they had not been given enough space for the research of Serbian culture, and they were eager to put things straight. The exhibition 'The Kosovo Legend in Folk Creativity', opened on the very eve of Vidovdan 1989, a day prior to the infamous speech of Slobodan Milošević on Gazimestan, on the occasion of the 600th anniversary of the Kosovo Battle,[25] can be interpreted as a sign of reorientation towards political activities and nationalist ideology of that time. In that sense, the exhibition about the Kosovo myth symbolically marked the end of the socialist era in the work of the Museum and announced the beginning of different and in many ways far more problematic times.

However, it should be mentioned that at the end of this period, and in sharp contrast to the increasingly pronounced ossification of the institution, one could also observe the arrival of young, fresh people who studied culture and cultural phenomena from a different, pronouncedly anthropological perspective.[26] Young curators, in spite of being marginalized to a certain extent by their mainstream-oriented colleagues,[27] presented in their exhibi-

[25] This historical event marked the beginning of a long process of subjugation of Serbia under the Ottoman power, which ended with the fall of Smederevo, the last Serbian capital, in 1459.

[26] This trend can be related to the penetration of new theoretical approaches in the study of cultural phenomena from the West, which could be discerned in Serbian ethnological scientific institutions (Department of Ethnology of the Faculty of Philosophy in Belgrade, Ethnological Institute and Institute for Balkan Studies of the SASA) in the late seventies and in particular during the eighties.

[27] To a certain extent, these new winds were, and still are opposed by curators with a long experience, who continue to have a strong influence in carrying out the professional policy of

tions a number of rural themes in the light of new and challenging scientific and museological approaches. Their exhibitions also displayed themes from urban life, sub-cultural groups, and from the up to then completely neglected aspects of rural and urban everyday life. Such fresh trends can best be illustrated by the exhibitions entitled 'Children's Toys as Status Symbols',[28] 'Children in Traditional Culture'[29] (focusing again on status and generation differences in rural culture), 'Football Fans' Kits and Accessories',[30] or 'Urban and Village Everyday Life'.[31]

Conclusion

While the overcoming of the pre-war legacies in the work of the Museum was often stated as a strategic goal of the new 'Marxist' approach, notably in the programmatic critiques offered by Špiro Kulišić, the results were at best partial, if not paradoxical. The most fervent ideological offensives often had quite 'conservative' outcomes, while at least some of the fine innovative work resulted from initiatives that had little to do with official 'ideologically correct' modernizing policies.

Thus, during the period of socialism the Ethnographic Museum continued to focus most of its research and exhibition activities on various aspects of folk or rural culture, primarily that of the Serbian nation, and only marginally on the cultures of other Yugoslav nations and ethnic minorities. Among the factors responsible for this paradoxical tendency, which flagrantly contradicted both official political goals and proclaimed institutional policies, one should single out the complicated and inhibiting federalist solution to the national question in socialist Yugoslavia, the effects of which were further aggravated by the adoption of the quasi-confederate Constitution in 1974.

Socialist ideological initiatives influenced the work of the Museum in other specific ways as well. Fervent criticism of anthropological theories coming from the West, advocated by indoctrinated ethnologists of Marxist orientation, conditioned curators to remain cautiously neutral, to focus mainly on pragmatic tasks, on the collecting, systematization, processing and

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the Museum, and who remain faithful to the traditional understanding of ethnology. They still impose a value system according to which work in an ethnographic museum primarily depends on expert knowledge and systematic presentation of rural artefacts.

[28] Exhibition posted in 1985 in the Manak's House, author Vesna Dusković.

[29] Author of the exhibition was Gordana Ljuboja (1988).

[30] Author of the exhibition was Ivan Kovačević (Department of Ethnology, Faculty of Philosophy), exhibition posted in 1986 in the Manak's House.

[31] Exhibition of photographs of authors Ranko Barišić and Ljiljana Gavrilović (1989), Manks' house.

conventional exhibiting of museum artefacts. In that period a neutral and pronouncedly descriptive 'method' was established, accompanied by a 'theoretical vacuum' that lasted for more than two decades.

On the other hand, the official ideology positively influenced other developments in exhibition activities. Obligatory participation of the Ethnographic Museum, together with the majority of other scientific and cultural institutions, in the social, economic and political development of Yugoslav society, in the so-called 'socialist building of the homeland', resulted in the increase of the number of exhibitions, the diversification of their topics, and their accommodation in unusual presentation spaces and the reaching of new audiences. Non-permanent exhibitions in the period of socialism became much more numerous, and started being profiled according to specific research themes. The Museum fund was opening out towards students and experts from other disciplines, such as architects, designers or film artists. Specially designed exhibitions of the 1960s left the halls of the Ethnographic Museum and 'went to the People'. Target groups of these 'visiting' exhibitions widened in order to incorporate pioneers, youth, workers and soldiers – the key strata of the 'new socialist society'. The Ethnographic Museum started to participate much more actively in the cultural life of Belgrade, Serbia and the federal state. Exhibitions toured elementary schools, factory halls, and entered the repertoires of various cultural events. Furthermore, in the periods from 1948 to 1952, as well as after 1966, the exhibition on the 'Folk Art of Yugoslavia', with the 'most beautiful pieces of folk creativity from all parts of the Socialist State', entered foreign exhibition premises and contributed to the positive image of 'socialism with a human face'. Unfortunately, such developments lost much of their momentum during the terminal crisis of Yugoslav socialism, which started in the late seventies and reached its climax during the second half of the eighties.

Rather unexpectedly, the first moves towards what is now called the 'anthopologization' of museum practices could be felt already in the 1960s, primarily in teamwork-oriented complex research projects which analysed changes in rural areas that resulted from forced socialist industrialization and urbanization. Paradoxically enough, some of these projects were initiated by museum 'cadres' that could boast of being the most orthodox amongst Marxist doctrinaires, and for whom the very mention of the word 'anthropology' was anathema. These early innovative trends in research and exhibition activities were 'revisited' in a more systematic and theoretically much more sophisticated way towards the end of the 1980s, when young curators, although marginalized by their senior mainstream-oriented colleagues, succeeded in presenting new materials from the fields of urban anthropol-

ogy, the study of sub-cultural groups and everyday life in vibrant and provocative thematic exhibitions.

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Chapter 14

On a Not So Well Tempered Marxism: Ideological Criticism, Historical Reconstructions, and a Late Return to Ethnogenesis in the Work of Špiro Kulišić

Gordana Gorunović

This paper takes the example of a single author, Špiro Kulišić (1908–89), to look at how Marxism was applied in the Serbian/Yugoslav ethnology. This individual case will interest us here insofar as an examination of Kulišić's professional biography and institutional activity, ideological involvement and scholarly work may reveal some fundamental properties of ethnology and the application of Marxism in the post-war period and within the political context of Yugoslav socialism. With the benefit of hindsight, we may argue that the 'Kulišić case', often thought to be closed once and for all, turns out to be far more complex in its significance and meaning. Hence to reopen it might result in a more comprehensive picture of ethnology under socialism and a more realistic appraisal of its legacy.

Until the 1970s 'Marxism' in Serbian and Yugoslav ethnology was a hybrid, derived from an encounter and blending of classical anthropological evolutionism with historical materialism. This strand of thought is conventionally known as 'evolutionary-Marxist' theory (Pavković, Bandić and Kovačević 1983), sometimes (pseudo-)Marxism, or simply evolutionism (Prošić-Dvornić 1997: 96). The sources of the evolutionary-Marxist conception Kulišić promoted in our ethnology may be found in Soviet ethnography (Kosven 1957, 1963) and the Italian ethnological-folkloristic school (Gasparini 1973).

The story presented here of the lone champion of a marginal theoretical paradigm – since dialectical materialism was just 'an appendix' in Serbian ethnology (Kovačević 2005: 14), as the received wisdom puts it – is potentially revisionist. The argument of marginality and sterility of Marxism in Serbian/Yugoslav ethnology is partly justified by the fact that this Marxism lacked any deeper theoretical reflection or elaboration, while its application boiled down to reiterating 'commonplaces' packaged in popular explanations taken from Engels or Soviet and Italian authors. The same argument

was then transferred to the entire opus of the author who is thought to be the one most responsible for such theoretical reduction and methodological simplification. Nevertheless, the subsequent demonization of Marxism–'Špirinism' is unable to explain some important points, such as: how a 'marginal' author could influence the shaping of the research agenda, the strategy of institutional development and the polemical atmosphere in ethnology from the 1950s to the early 1980s, as Kulišić did; and why at least one, and probably the most problematic portion of his work (the discourse on Montenegrin ethnogenesis) survived both the author and socialism. Independently of the theoretical and ideological sign, Kulišić's approach, with its topics and problems, resonated among a number of admirers and followers, who in their turn occupied important posts in our discipline until very recently.

Kulišić's professional biography reflects the social circumstances that favoured the mobility of intellectual cadres in the two Yugoslav states – both the kingdom of Yugoslavia and the Socialist Federal Republic of Yugoslavia. One of the peculiar features of Yugoslav socialism as compared with the Soviet Union and other people's democracies was a looser connection between party power centres at different levels (such as the federation and the republic). The permeable inter-republic boundaries enabled a relatively unhampered flow of intellectual cadres and scientific cooperation between individuals and institutions. This was particularly true of the southern republics, which, in addition to mutual cultural and historical ties, as well as geographic contiguity, were linked together by higher education policies (in the case of ethnology, by the dependence on Belgrade University). The 'heroes' of our story of ethnology in the early socialist period, Špiro Kulišić and Milenko Filipović,[1] are among the most prominent professionals who, until the 1970s, decisively contributed to shaping the face of ethnology and museology in Bosnia-Herzegovina and Serbia, while at the same time leaving a trace in Balkan studies as well.

Beside limitations resulting from insufficient or unavailable sources, our understanding of the past is also burdened by the tendency to interpret professional antagonisms and disputes through the lens of the supposedly repressive 'spirit' of socialism/communism and its ideological cleavages. It

[1] Born in Podlugovi near Sarajevo in 1902, Filipović studied (1921–5) and obtained his Ph.D. ('The Ethnic Past of Our People in the Region of Visoko, Bosnia', 1928) in Belgrade. In 1930 he was appointed Assistant Professor, and in 1938 Full Professor at the Faculty of Philosophy in Skopje (Macedonia). In 1939 he founded the Ethnological Society in Skopje and launched its organ 'Ethnology'. After the 1950s his life and work were most closely tied to Bosnia-Herzegovina: he was professor at the Faculties of Philosophy and Natural Science, member of the Scientific Society of BH, director of the Institute of Balkan Studies and member of the Academy of Sciences of BH since 1966. He died in Belgrade in 1969.

is sometimes difficult to uncover the direct political input in events and separate unambiguously political interests from the personal motivations of the actors involved, who were at once bearers of discourses, careerism, personal vanities and professional rivalries (as it was the case in the 1950s debate on functionalism and contemporary ethnology between Kulišić and Filipović).

Depending on the historical moment, social context and current political objectives, the instrumentalization of historical materialism followed the tumultuous dynamic of the societal system: from the clash with 'class enemies' of the party and the people to the pursuit of particularistic interests and national programmes of certain ethnic groups. In the Cold War period, criticism of Soviet science and Western paradigms was intended as a means to purge home ranks and establish historical materialism as a class-conscious and ethnically neutral paradigm. In the period after the death of Josip Broz Tito and the bursting of the Yugoslav crisis, in the hands of Kulišić and his political 'mentors' the same paradigm (the discourse of Montenegrin ethnogenesis as a sort of ethnicization of ethnology) benefited precisely those processes against which it was originally introduced as a remedy – the politicization of ethno-national identity, Montenegrin separatism and secessionism, and the final disintegration of the common state.

Biography and Institutional Activity

Kulišić was born in Boka Kotorska (then within the Austro-Hungarian empire) in the south of today's Montenegro, in 1908. This was the year of the political crisis in the Balkans provoked by the annexation of Bosnia-Herzegovina, which would instigate further escalation of the Serbian and Yugoslav problems (Cvijić 1987 [1908]). He studied ethnology at the Faculty of Philosophy in Belgrade (1927–33) with the 'founding fathers' of the Serbian academic ethnology Jovan Erdeljanović and Tihomir Đorđević. His teaching jobs took him from the kingdom's capital to Croatia and Bosnia. On the eve of World War II he became a communist, and during the war he joined the partisan movement. After the war, he was a leading specialist in Bosnia-Herzegovina for two decades. He worked in the Ministry of Education and Ministry of Science of the People's Republic of Bosnia-Herzegovina (as the official in charge of scientific institutions and associations), in the National Museum, and the Institute of Folklore Studies in Sarajevo, where he coordinated regional research projects. For a short time he was a part-time teacher at the Faculty of Philosophy of the Universities in Sarajevo and Belgrade. The curiosity connected with his unsuccessful university career is that he never obtained a Ph.D. degree. In the early 1960s he moved to Belgrade to take up the post of director of the Ethnographic Mu-

seum. Although he withdrew from institutional life comparatively early (retiring in 1963), he took part in public life through published writings until the early 1980s. He died in 1989, a couple of months before the fall of the Iron Curtain and the Berlin Wall. The year of his death coincided with the explosion of the crisis of the Yugoslav federal state and the one-party system.

The creation of the separate republic of Bosnia-Herzegovina (as a federal unit within the Federal People's Republic of Yugoslavia, FPRY) was a political decision that had to be legitimated, including the development of institutional infrastructure in higher education[2] and science.[3] During the first post-war years, the needs for ethnological research in the Republic by far surpassed the available financial and personnel resources. Everything had to start from scratch: work had to be organized, ethnologically trained staff brought in from Belgrade, and a feasibility study composed on the prospective development of the Ethnographic Department of the State Museum in Sarajevo, which soon regained its old name of the National Museum of Bosnia-Herzegovina. The two authors who worked most actively on this study were Kulišić, State Museum director (1947–50), and Filipović, 'very well acquainted with ethnological phenomena of the Bosnian-Herzegovinian region' (Buturović and Kajmaković 1988: 156).

Specialists in Bosnia-Herzegovina were among the first in Yugoslavia to try out team work. There were a variety of reasons for such a choice, like the complexity of research problems, and the necessity to harmonize divergent views espoused by the leading authorities from different university centres (Filipović-Fabijanić 1970: 157). At the end of the 1950s, under Kulišić's leadership a new conception of research work was introduced within the Institute of Folklore Studies and the Ethnological Department of

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[2] The Faculty of Philosophy in Sarajevo was founded in 1950. Its Chair in Geography included courses in ethnology as well (at first as compulsory and later as optional) until 1962, when the Chair was abolished. Since then ethnology has not been taught at all. For a brief period in the early 1950s Kulišić also taught at this Chair, to be replaced by Filipović as Full Professor in anthropogeography and ethnology (1955–62).

[3] In 1951 the Scientific Society of Bosnia-Herzegovina was founded, with the aim of coordinating scientific work in the Republic. In 1954, the Institute of Balkan Studies was established as one of its branches. During the eight years of existence the Institute housed comparative and interdisciplinary research in languages and cultures of all Balkan peoples. From 1955 on Filipović was a full member of the Scientific Society and the director of the Institute of Balkan Studies (1959–62). The Institute's most voluminous and ambitious project, and a contribution of lasting significance, was the study of a central problem in Balkanology – the medieval Vlach *katun*. The Institute of Balkan Studies was closed in 1962, and its activity was taken over by the Centre for Balkan Studies of the Academy of Sciences and Arts of Bosnia-Herzegovina, focusing on Illyrian heritage. In 1965 Kulišić became a permanent research fellow of the Centre.

the National Museum in Sarajevo. Its chief tasks and goals were: the inclusion of the generally scarce trained personnel in new, 'theoretically grounded, long-term museological and especially scientific research projects' (Palavestra 1990: 192) geared into targeted team-based ethnological and folklore studies; the study of ethno-cultural traits and formation processes of regional groups of the three majority peoples (Serbs, Croats and Moslems).[4]

The methodology of complex monographic studies consisted of preparatory work and fieldwork (see Buturović and Kajmaković 1988). The chief novelties were team work, field research, systematization of ethnographic–historical corpus, and collective monographs.[5]

It is difficult to say whether party organs at the republic level had any direct influence on scholarly research, by either commissioning or initiating particular projects, since there are no incontrovertible data on this. The example from the following quotation suggests the possibility of seemingly informal and personal initiatives, one among them resulting in the invention of ethnic groups and their tradition:

> Kulišić invented *White Šokci* and *Ikavian Serbs*. Some time around New Year's Eve the then republican official Jovan Marjanović asked Kulišić in a conversation about *White Šokci*. Kulišić had no idea what it was all about, but he managed to dodge the answer. He immediately called upon me and ordered me to go to Prnjavor and dig out who the '*White Šokci*' were. I had to do as he said; there I found out that this was the autochthonous Croatian population that had retained their traditional white costume, after which they were named *White Šokci*.
>
> And as for the 'Ikavian Serbs', three of us colleagues had to go into the field, to the mountain of Vlašić, in the middle of winter, treading knee-deep snow, trailing a horse that was carrying our baggage. There we found out that these Serbs really had an Ikavian word or two, but this stock of Ikavian words could simply have been taken from the neighbours. Since in Livno there was a group of Serbs with Ikavian elements in their speech, the category of Ikavian Serbs was invented, together with the project that had to prove their existence,

[4] In the early 1960s the main task of ethnology in Bosnia-Herzegovina was defined as 'the study of ethnic processes and ethnic formations' in the Middle Ages, so that over time a 'comprehensive scientific study of the national development' of Bosnia's peoples would become possible (Kulišić 1961a: 323, see also 1961c).

[5] These ethnological and folkloristic studies shed light on the medieval formations and processes in the shaping of the contemporary Croatian population at Neum and its surroundings (1959), of Croats, Serbs and Moslems in Livanjsko Polje (1961), and Serbs in the village of Imljani at the mountain of Vlašić (1962).

> entitled Livanjsko Polje and Its Surroundings (R. Kajmaković, interview).

Kulišić himself was more inclined to armchair study than to fieldwork. In the account of his closest assistant:

> Kulišić was no fieldworker. Once, just after the war, he went into the field accompanied by two soldiers. People in the villages were frightened; there were quite a few remaining Chetniks and Ustashas around in isolated villages at the time, and a visitor from town escorted by armed soldiers provoked additional fear.
>
> Our work proceeded in such a way that he would design the project outline and distribute work tasks. Customs was my area. When the collection of data and research were finished – we spent three months in the field every year – I had to hand over to him a neat, typed paper. Then he would say: 'I will take this, and you take that.' He would use only those data that suited him. All that did not fit into his preconceived 'story' or idea was ignored. Yet at the same time he prevented anybody else from using these data either (R. Kajmaković, interview).

As the director of the Ethnographic Museum in Belgrade (1960–3), Kulišić introduced significant changes in the organization of museological and scientific work (Kostić 1992: 389–90). Upon his initiative, an educational division was created, departments for studying textiles, economy, and sociability, as well as documentation and conservation services. In the field of scientific research, Kulišić consistently implemented his conception of complex monographic studies of regional wholes, now in the territory of Serbia (north-western Serbia, focusing on the Serbian-Vlach ethno-cultural symbiosis), which required team members to specialize in different areas (sociability, customs, material and spiritual culture, ethnogenesis, etc.). The museum has retained the same structure to this day.

Ideological Engagement: Critique of Past and Present

In the first decade of socialist transition, the rhetoric of the new era and new system promulgated a sharp break with the past and an 'ideological and theoretical clearing up' (Djilas 1949; Kulišić 1953a). The 'theoretical clearing up' implied a critical analysis of the results previously achieved and methods utilized, from the standpoint of Marxism–Leninism, and a concrete application of the principles of the latter doctrine in studying national history and culture of all Yugoslav peoples and nationalities. According to the ill-articulated conception of the new, trans-ethnic science, the socialist ethnology was from that moment on supposed to serve the goal of providing a 'correct explanation' of the past and a 'correct ideological and cultural

upgrading of our people' (Kulišić 1953a: 30). Its positioning as regards Western anthropology and Soviet ethnography proceeded through ideological critique of the political functions of bourgeois science and the 'revisionism' (or 'reactionary deviation') of contemporary Soviet ethnography. The critique was primarily aimed at the heritage of the local 'bourgeois science' – the Serbian anthropogeographical and ethnological school of J. Cvijić and J. Erdeljanović – and at contemporaries in ethnology, archaeology and physical anthropology (Kulišić 1953c) who were deemed 'suspicious' because of their pro-Western theoretical orientation and/or political past.

This new trend was announced in Kulišić's propagandistic 1953 paper 'Ideological and Theoretical Grounds for the Development of Our Ethnographic Museums', which contains a critique of museological practice in the inter-war period. The first expression of 'ideological orthodoxy' (Naumović 1999) was his attempt to introduce a new name for the discipline, 'ethnography'[6] instead of the usual 'ethnology' that enjoyed consensus in academic and specialist community (Rusić 1954). According to Kulišić, 'ethnography' is an 'essentially social science whose development is closely connected with the development of the national question and directly conditioned by the basic tendencies in the development of contemporary society'. Its primary task is to 'study the emergence of particular peoples and their culture' (Kulišić 1953a: 27).

The argument for introducing the new name was mechanically transplanted from the Soviet ideological repertoire: ethnology was a bourgeois reactionary science,[7] which should be rejected even at the level of terminology. After the 1948 Cominform Resolution, when the Yugoslav communists changed drastically their course towards the Soviet Union and encouraged open critique of Stalinism, such a belated reflex of Stalinist–Zhdanovist critique of ethnology was paradoxical. As soon as the mid 1950s the author himself renounced it and definitively opted for the name 'ethnology'. In the early 1960s he criticized the solutions he had previously advocated (Kulišić 1961b: 20–1).

Bearing in mind the internal political situation and the position of FPRY towards the USSR and other East European countries, it is quite understandable that Kulišić, in a 1953 paper, attempted to distance himself from Soviet practice and Soviet science. In this criticism he argued that

[6] In this Kulišić was not alone within the Yugoslav space: the Slovenian organ of the Ljubljana Museum of Folk Culture *Etnolog* [*Ethnologist*] was the first to change its name into *Slovenski etnograf* [*Slovenian Ethnographer*] in 1948 (Rusić 1954: 60–1).

[7] The label 'ethnology' was censured because of the 'retrograde teachings' of West and Central European scientists (e.g. Wilhelm Schmitt) and rejected at the congress held in Leningrad in 1929 (Rusić 1954: 59–60).

Soviet ethnography was undergoing a 'serious crisis' caused by: 'profound contradiction between its proclaimed principles and actual Soviet practices' (the 'hegemonist policy of the Soviet leadership'; the 'policy of restricting the free development of peoples'; the 'phenomenon of genocide'); absolutization of Soviet and Russian science; and a revival of chauvinist pan-Slavic ideology.

The contextualization of the Serbian bourgeois ethnology, originating from within the nineteenth-century national–historical science, was supposed to confirm the assumption of its class and political partiality, and ethnocentric premises.[8] Ever since the creation of the kingdom of Yugoslavia with its 'centralist' and 'unitarist' regime, Kulišić writes, the development of ethnology was subject to the 'policy of national oppression' enforced by the ruling bourgeoisie.[9]

The 'hegemonist policy of the regime' reflected also on the methodical principles of museology, derived from dominant 'bourgeois' theories – evolutionism and the cultural cycles theory (the so-called German and Austrian cultural–historical school). The basic characteristics of museum work were: underestimation of ethnic peculiarities and cultural–historical development of minority groups (Montenegrins, Macedonian Slavs); the tendency to present the cultures of majority nations in the state-political community as an integrally Yugoslav whole (Yugoslav and Serbian ethnocentrism); methodological 'formalism' and 'anti-historicism' (Kulišić 1953a: 28).

Yugoslav ethnology in the 1950s was characterized by several recognizable features: a focus on rural traditional culture; 'collectionist empiricism'; introduction of the model of ideologically correct doctrine of society and history (especially of the 'primeval society'); and the rise of an alternative research strategy (proto-functionalism) which the ideologized, (pseudo-)Marxist current considered alien, potentially subversive and rival.

The alternative to ethnology as the science of the rural setting and traditional culture ('antiquarian ethnology', of the scholarship of 'living antiquities'), to whose persistence Kulišić continued to contribute in his later years, was formulated as early as the mid 1950s by Milenko Filipović, probably the most prolific Serbian ethnologist and Kulišić's most prominent rival. This proposal is all the more remarkable since it came in a period of radical social change, when most ethnologists thought that the phenomena

[8] In later years, too Kulišić repeatedly invoked the thesis of 'ideological-political tendentiousness' of Serbian bourgeois ethnology and anthropogeography (1966: 102, 104; 1967b: 205).

[9] This interpretation was based on the doctrinaire stand of the Comintern and the Yugoslav Communist Party that the kingdom of Yugoslavia had been the 'dungeon of peoples', because of the chauvinist (Greater Serbian) ideology and practice of the ruling dynasty and the Serbian bourgeoisie.

threatened by extinction should be recorded and 'the picture of the ideal village society reconstructed', while the 'questions of transformation, social development and current problems the society was facing were relegated to sociologists' (Prošić-Dvornić 1997: 73).

Contrary to Kulišić's predominantly armchair working style, Filipović was 'starting from empirical evidence, from establishing facts and framing the problem' (Filipović-Fabijanić 1970: 158). He promoted social ethnology or ethno-sociology (Filipović 1956: 146), the functional approach in studying contemporary culture, and application of ethnological knowledge in implementing social actions and in solving practical issues.

This realistic alternative was motivated by objective reasons of theoretical and social–practical nature: by the awareness of the necessity for modernizing the theoretical and methodological apparatus of Serbian ethnology and approaching current trends in Anglo-Saxon and Anglo-American anthropology, whose preoccupations Filipović knew and some of them even shared. Industrialization, urbanization, country-town migrations, spread of literacy and other processes were affecting, albeit unevenly, all segments of society and contributing to the transformation of traditional patterns in the life of rural populations.

In order for home ethnology to be able to meet the challenge of modernity, it had first to get rid of its obsession with exoticism, primitivism and archaism, idealized models of folk (peasant) life and culture, and the imperative of 'ideological orthodoxy'. One of the ritual expressions of this orthodoxy and a new 'genre in ethnographic writing' (the 'ideological denunciation of colleagues', Naumović 1999: 58–9) was the critique of functionalism and its propagators in domestic scholarship (Kulišić 1955a, 1956a). It was precisely then that the 'universal' method-formula of ideological and political defamation of the opponent was invented, based on an equation between the categories of 'anti-historicism', 'functionalism', and 'imperialism'.

Seeking to depict the standpoint of anthropological functionalism as being intimately tied to the practice of Western societies in the neo-colonial epoch (imperialism as the highest stage of capitalism), Kulišić drew on the Soviet critique of British applied anthropology (Potechin 1948). He argued that the 'class essence' of the functional theory consisted of pragmatic tasks – to preclude the awakening of national consciousness in the colonies and to facilitate functional integration of the latter into capitalist society; 'in other words, to ensure colonial exploitation' (Kulišić 1955a: 298). And it was precisely this purpose, wrote Kulišić caustically, that bestowed 'topicality' and 'trendiness' on the principles and objectives of the functionally oriented anthropology, which rightly took on the attributes of 'practical' and 'applied'.

The immediate cause that prompted Kulišić to compose this 'piece unusual in its contents as well as its manner' was Milenko Filipović's article 'Ethnological (Ethnographic) Work in Bosnia-Herzegovina' (1955).[10] In this text Filipović sketched in general terms and 'in a style accessible to the non-specialist readership' a critical overview of the earlier ethnographic–ethnological work in the territory of Bosnia-Herzegovina and presented his own ideas on how further research should be conducted.

Because of temporal indeterminacy, regional unevenness and disparateness of ethnographic data, Filipović pleaded for 'critical' research that would take strictly into account the chronology of data, geographical spread of the phenomenon, and its role ('functional relation'). At the same time, he did not deny the need for additional efforts in the direction of historical reconstructions. Within this research agenda he formulated a host of problems belonging into the domain of historical ethnology and cultural history (Filipović 1955: 214–15).

Today's ethnology, that Filipović then proposed to be preceded by the adjective 'social', after the model of 'social anthropology':

> looks at phenomena in the life of the people in all their dynamics, observes and studies processes, their conditions and consequences; and any data collection is subordinated to these goals; while in earlier times objects and descriptions were being collected just for their own sake' (213–14).

Stressing the new possibilities opened up for ethnological research by fresh paradigms, he set forth explicit demands for synchronic studies of the current condition in folk life and culture, and for viewing the cultural phenomenon from its social side (Rakić 1970: 178). He stressed that Bosnia-Herzegovina was a rewarding region

> … also for those scholars who deem ethnology to be a science of living antiquities. For, in our ranks there are also such antiquarian ethnologists (ethnographers) who devote all their attention to antique forms of tools and technology, age-old customs and beliefs as supposed survivals, etc. (*ibid*.: 213).

Having read these words, Kulišić felt called upon and asked to speak in the name of these 'antiquarian ethnologists (ethnographers)' searching for

[10] The article was published in Pregled [Review], the Sarajevo journal for social issues that often served as the arena for scholarly and specialist debates. Filipović (1956: 143) was 'ready for discussion, and even wanted it, all the more so as our ethnology, especially the Serbian one, is at the moment in a state of slumber … As much as Š. Kulišić's initiative must be welcomed for broaching several theoretical problems in his article, the way he did it is lamentable.'

'supposed survivals'. And speak he did, in a paper on functionalism.[11] He seemed to be personally hurt by the fact that his colleague brought into question the impassioned devotion to 'age-old customs and beliefs'. Filipović tried to smooth it over in a subsequent article (1956: 148):

> It is not antiquarian ethnography if one is engaged in real study of archaic elements, including 'survivals'. This sort of work is held in high esteem everywhere. My attitude to this sort of work, that is apparently Kulišić's chosen area of specialization, is very well known to him from my highly favourable review of his essay 'The Origin and Meaning of Christmas Ritual Bread among South Slavs' (1953, see Filipović 1954: 166–8).

This statement did not help; a rift ensued, and cooperation and friendship were broken between the two leading Serbian ethnologists, whose works had influenced a whole generation of specialists. The debate on tendencies in contemporary ethnology and the most urgent tasks of domestic science, initiated by Filipović in the mid 1950s in the hope of launching a true dialogue, failed soon and, thanks to Kulišić's manner of debating, spilled well over the confines of a scholarly polemic. The unofficial version of events furnished by Kulišić's assistant from the National Museum sheds some light on the personal dimension of their ideological conflict:

> After the war, since the founding of the Faculty of Philosophy in Sarajevo in 1950, Kulišić taught ethnology at the Chair in Geography. His post was conditioned upon obtaining the Ph.D. degree. He chose the topic – Christmas ritual breads, with Dr. Dušan Nedeljković as supervisor. Privately, Kulišić knew Milenko Filipović, they were friends; they used to pay each other family visits, and Milenko was helping him. But Milenko and Dušan Nedeljković had some 'unsettled accounts' from the past, from the pre-war period when both worked at the Faculty in Skopje. Filipović apparently forgot to mention to Kulišić a small piece by Nedeljković on *kravaj*, which Kulišić was supposed to consult when working on his dissertation. The conflict between Nedeljković and Kulišić broke out when Špiro, short-tempered as he was, refused to do so and to edit the already written text. The result was that he withdrew his dissertation.
>
> After that he went to Split and for some time taught at the Higher Educational School there. During this time Milenko Filipović

[11] He accused Filipović of 'total anti-historicism'; of limiting himself, in the form of 'modern ethnology' and 'genuine science', to 'the present and exclusively to the present', propagating thereby 'reactionary' ideas and methods of the functional school which gives a 'practical contribution to the enslavement of colonial peoples', etc. (Kulišić 1955a: 298–302, 1956a: 463-6).

> worked at the Sarajevo Faculty of Philosophy, at the Chair in Geography, and was also a consultant in the National Museum. Špiro returned to Sarajevo, expecting an appointment at the Faculty, but there was no place for him there. This was obviously a sufficient reason for him to have a personal requital with Filipović, a requital which had the outside form of a 'scholarly' polemic on functionalism. Kulišić had to work in school again, he was the headmaster of some school, but he wanted to come back to the Faculty. Then he sent a letter to Milenko Filipović, in which he advised him 'as a friend' to give up Western literature and turn to the Soviet one. He had already published the article on the anti-historicism of the functional school in ethnology. It was then that the campaign against Filipović started (Radmila Kajmaković, interview).

After that Filipović 'refrained from engaging in public debates on theoretical issues' (Rakić 1970: 179), but in actual research he consistently deployed his principles in the area of ethno-sociology and ethno-history, combining elements of functional and historical approaches.[12] Although he 'never really abandoned the classical East European paradigm' (Prošić-Dvornić 1997: 76), his theoretical–methodological orientation was in its time more modern than the mainstream in most East European ethnologies (Halpern and Hammel 1970). 'Unfortunately, few Serbian ethnologists followed his lead', Mirjana Prošić-Dvornić concludes resignedly.

At that particular time and in the context of Serbian ethnology, Filipović's pro-functional orientation constituted a realistic alternative and a creative challenge to historical materialism and evolutionism. Maybe his critics recognized or felt the subversive potential of the new paradigm – 'the threat to their own discipline and its subject matter', to paraphrase the words of Prošić-Dvornić, or just to their personal professional and political positions. They were well served by Filipović's political status of *persona non grata* that closed the doors of the Belgrade University to him, and therefore the chance to establish his own ethnological school. In this way, whether 'grey or yellow, the devil' of functionalism (Kulišić 1956a: 464) was, at

[12] For example, under his leadership the Sarajevo Institute of Balkan Studies organized an interdisciplinary symposium on the medieval Vlach *katuns* – their socio-demographic and economic structure, position towards the village organization within the feudal society, and role in the creation of the Dinaric tribes in the late Middle Ages. In addition to Filipović, participants included the historians Branislav Djurdjev (professor) and Desanka Kovačević (assistant professor) from Sarajevo, and the anthropogeographer Jovan Trifunoski (professor of Skopje University) as lecturer. Their presentations and discussions, as well as contributions of guest participants, were published in the collective volume *Simpozijum o srednjovjekovnom katunu* [*Symposium on Medieval Katun*] (1963)

least for some time, neutralized by magical formulae, so that the imperilled community of the 'orthodox' could buttress their faith.

The liberalization of the political climate in Yugoslavia during the 1960s, opening up towards the West and to some Western paradigms, rather strongly felt in domestic philosophical and sociological circles, had a negligible impact on ethnology. In the words of one of the fiercest critics of domestic academic ethnology of the period, 'the best example of avoiding any contact with philosophy, both substantially and formally, is the Belgrade chair in ethnology, which avoids even sociology' (Gluščević 1966: 343).

The Marxist current, flimsy in professional terms but occasionally aggressive, decided to ignore the international development of Marxism itself, as well as its philosophical and anthropological reception in the country. In response to its manoeuvres and attacks against others, the Marxist current was taken to task for advocating vulgar materialism, poor knowledge of classical Marxism, and misunderstanding of 'the possibilities Marx had opened for anthropology' (Gluščević 1966: 343).

After the debate on 'genuine' ethnology, its subject and its present tasks was opened, new actors joined in (Gluščević 1966 *vs*. Kulišić and Barjaktarović 1964). Manojlo Gluščević, who defended his doctoral dissertation in 1960 (Gluščević 1964) at the Faculty of Philosophy in Sarajevo with M. Filipović as supervisor, published an article entitled 'Ethnography, Ethnology, and Anthropology' (1963: 21–50), one of the first explicit criticisms of Erdeljanović's conception of ethnology as the science of nations, ethnic characteristics and ethnogenesis. This was also an early attempt at anthropologizing domestic science: the author defines ethnology in an anthropological vein, as the study of man in all aspects of his existence (biological, social and cultural).

The article prompted Špiro Kulišić and Mirko Barjaktarović, then a professor at the University of Belgrade, to write a polemical pamphlet (1964: 348–58), whose rhetoric essentially boiled down to the defence of the good old ethno-historical perspective in ethnology and to the application of the universal formula of 'anti-historicism'. They rebuked the author for neglecting a basic aspect of man's existence – historicity, and for denying ethnology the character of historical discipline. Yet the real target of Gluščević's criticism (1963: 27, 1966: 340–1) was *putative history* in ethnology which 'conjures up on the basis of unchecked and highly unreliable oral tradition' and at best can be nothing more than a 'hypothetical reconstruction of past events'. The critics 'on duty'[13] disputed the view that anthropology was the

[13] Gluščević (1966: 331, 343) criticized the two ethnologists for their lack of familiarity with the English language and Anglo-Saxon literature, for scholastic logic in their argumentation, and for repeating the commonplaces of vulgar Marxism ('they speak in the name of Marxism

most comprehensive science of man in nature, society and culture. They claimed that ethnology was not a science of culture and society since 'it studies the emergence and development of ethnic communities, their social institutions and their culture' – as if ethnic communities were not social communities! (added Gluščević 1966: 338). They also set forth the thesis of the non-homogeneity (i.e. class character) of national culture. Gluščević retorted that these authors when studying the so-called 'folk culture' never took into account the totality of a people's culture, but just of a part of it, that is, the peasants. Hence, the alternative was either to study the past of certain backward strata or to adopt a modern concept of ethnology, that is, anthropology.

However, neither the modern conception of ethnology nor the very term 'anthropology' were innocent or neutral. In spite of his knowledge of French and Italian, and brief visits to foreign countries, Kulišić's distancing from anthropology and sociology was drastic. He challenged the concepts of social and cultural anthropology and the very possibility for the existence of anthropology as a global science.[14] He was also sceptical about the study of contemporary socio-cultural phenomena and working-class culture ('and perhaps what is actually meant by the name of ethnography is sociological surveys...') within Soviet ethnography, concluding that 'welding together ethnological and sociological problems and methods can only aggravate our difficulties in research' (Kulišić 1961b: 21).

Such rigidity and conservatism of the 'Marxist' branch of Serbian ethnology partly accounts for the unpopularity and failure of historical materialism[15] within the discipline, which in turn produced sporadic resorting to Filipović's alternative or escaping into the 'enemy ranks' of socio-cultural anthropology, informed by humanistic Marxism and *praxis* philosophy that was promoted by Prof. Zagorka Golubović. Unaware of his own contribution to such a result, Kulišić (1974a: 26) thought it pernicious

> that generations of students could complete their studies without really getting to know anything about a creative application of his-

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of which they are equally ignorant'). He accused them of 'camouflaging their ignorance by ideological sensitivity', of using the method of ideological–political discreditation of opponents, etc.

[14] For instance, he complained that Levi-Strauss' understanding of anthropology (Lévi-Strauss 1989 [1958]: 343) involves, 'quite unnecessarily, also those phenomena and tasks that naturally belong into the domain of sociology, as a separate scientific discipline' (Kulišić 1961b: 8).

[15] Kulišić was openly criticized for not being able to use the historical–dialectical method or the results its application yielded. His 'historical–dialectical method', which he so much 'bragged about' was 'neither historical nor dialectical', but rather consisted of 'naked quotations from Marxist authors' and 'miserable platitudes' on the material and social conditioning of religion (Djurić 1974: 28).

> torical materialism. In the same way ethnologists obtained their Ph.Ds and M.As, and some of them even got important positions in ethnological institutions ...

Describing the situation in Serbian ethnology as a 'void' and 'autarchic closure', he saw the reasons for such stagnation in the absence of scientists of the calibre of J. Cvijić, T. Đorđević and J. Erdeljanović; marginalization of Marxism (Kulišić 1973a); lack of a 'clear and consistent theoretical orientation and methodology'; and discrimination against the discipline (administrative-political 'tutelage').

> Various 'internal' committees were formed, like for instance at the Faculty of Philosophy in Belgrade, playing the role of some sort of tutoring agencies. Or external committees were put together in a purely bureaucratic fashion, entrusted with assessing and channelling the work of ethnological institutions in Serbia. I was also a member of such committees, and in the end I refused to take part in this job any more, since no administrative, tutoring commissions can help the development of a science, nor is this a path to recovery (Kulišić 1974a: 26).

Classical Topics and Grand Questions

Kulišić's research work bore the sign of continuity with classical topics, the 'grand' questions of ethno-history, and applied research methodology. There are three thematic clusters characterizing Serbian ethnology since its academic beginnings in the early twentieth century that also represent the core of Kulišić's interests: tribal organization in the Dinaric region; ethnic history and the relations between Slavic and autochthonous Balkan populations in the Middle Ages; and popular religion among Serbs and Montenegrins.[16] His research work built on the tradition of ethnographic and ethnological study of the people (peasantry) and traditional culture, marked by the long-lasting dominance of the historical–comparative and genetic approach as the most general theoretical and methodological framework within national science (Pavković *et al*. 111–12).

In his work on reconstructions, Kulišić followed the theory of matriarchy and patriarchy as two universal successive stages with the two corresponding forms of the 'primeval gentile community'; the transition from one to the other is a 'historically necessary' and 'lawful' process. Applied to

[16] A sizeable and perhaps the most valuable part of Kulišić's opus comprises his works on religion – this at least was the opinion of the late Prof. D. Bandić (personal communication), a leading authority in the field. Kulišić drew upon and harmoniously continued the tradition of Serbian ethnology and folklore studies in reconstructing pre-Christian religion, mythology and customs (see Kulišić 1953b, 1970a, 1970b, 1979).

local ethnographic data, this kind of 'putative history' automatically entailed the rigid assumption that the forms of gentile organization and kinship institutions, popular religion and customs among South Slavs had a matriarchal origin. As a consequence, ethnographic research had to be geared into the search for 'surviving' elements, relics of archaic relations and concepts in social practice and the symbolic sphere, while the analysis had to focus on interpreting their origin and 'secondary' development.

Contrary to Kulišić's main premise, Yugoslav historiography generally believed that South Slavs were never 'in the stage of transition from matriarchy to patriarchy'. The Slavic tribes that in the early Middle Ages arrived into the Danubian basin and the Balkan peninsula had, according to historical sources (see Curta 1998), patrilineal-patrilocal family and the so-called military-democratic social system.[17] The Slavic colonization of the Balkans coincided with the highest stage in the development of their patriarchal tribal organization, in which the 'seeds of the future class society and state building' were already present (Djurdjev 1954: 209; Čubrilović 1974: 50). The processes of economic differentiation and decline of the 'gentile basis' of the tribes, probably unleashed already during the migrations, military campaigns and alliances, escalated in the Balkans when the 'territorialization' stage set in.

One of the 'grand questions' of domestic ethnology and historiography, and one that still awaits a conclusive answer, is the explanation of two interconnected processes: the disintegration of feudal societies, and the formation of the tribal structure in the Dinaric region in the late Middle Ages (fourteenth and fifteenth centuries). There is still no theoretical–analytical framework that would successfully connect the conditions of locally specific cores and the effects of various factors in the formation of tribes. Among these factors the following are most often cited as decisive: ecological (cattle-breeding economy that requires seasonal migration between winter and summer pastures); ethnic and demographic (migrations of the population within the Dinaric and neighbouring regions); socio-political and historical (crisis of the feudal order of local feudal states; Turkish invasions).

For this reason the old debate between 'ethnographers' and 'historiographers' on the emergence and origin of tribes (the continuity theory vs. the Vlach-*katun* theory) has remained unresolved to this day. The theory of

[17] The term and concept of 'military democracy' was coined by Morgan (1981 [1877]: 204). In Engels' interpretation (Engels 1979: 115), military democracy is a specific organizational structure of gentile society that emerged when war and organization for warfare became 'regular functions of a people's life', and the plundering of neighbours 'a constant branch of the economy'. The main organs of this system were the military leader (rex, basileus, thiudans), council and popular assembly.

historical *continuity* of the Slavic tribal system[18] (Erdeljanović 1921; Cvijić 1966), emphasized the ethnic dimension of the process of repatriarchalization: the renewal of the tribe and reinforcement of the 'patriarchal regime' were understood as being in the function of 'ethnographic rejuvenation' and ethnic restructuring/'reshaping' of the Serbian people, in the political vacuum that ensued after the collapse of feudal states and Turkish conquests.

In historiography, proponents of this theory have been criticized for seeking the origin of tribes and the 'roots of tribal institutions' in the presumed Slavic background, neglecting historical facts (Djurdjev 1954: 209). The rejection of Erdeljanović's evolutionist generalizations (the theory of the Slavic background and two stages in tribal development),[19] but affirmation of his ethnographic results in studying specific tribes (see Erdeljanović 1981) have resulted in the conclusion that (paradoxically) the ethnologist, working in the field and on the material of popular oral tradition, proved the thesis formulated by historians – of the 'delayed shaping of Montenegrin and Highland tribes' (Djurdjev 1981a: 115). Though opinions are divided as to the political motivation underlying Erdeljanović's work (Yugoslavism or Serbian ethnocentrism?), his studies of tribal society are today considered the most complete and 'unsurpassed' – 'by breadth and depth of grasp, by originality and novelty' (Djurdjev 1981a; Pavković 1995); he is also seen as a precursor of political ethnology/anthropology (Pavković 1995: 274).

The main contention of the post-war Marxist historians, the upholders of the so-called Vlach-katun theory,[20] was that Montenegrin, Highland and Herzegovinian tribes had emerged towards the end of the Middle Ages and at the beginning of the Turkish period from the katun organization of Vlach cattle-breeders. According to their assumption, the medieval Vlach-

[18] Jovan Erdeljanović was the first professional Serbian ethnologist who in the early twentieth century began a systematic study of tribal life. Evolutionist and, more narrowly, Morganian, he – like Lewis Morgan himself (1981 [1877]) – found a lively tribal organization and tradition, which enabled him to engage in immediate fieldwork. On the basis of many years of ethnographic and historical studies, he concluded that there had been two stages and two ethnic layers in the development of tribes.

[19] The so-called 'older' formation belongs to the period from Serbian settlement to the founding of the Nemanjić state, while 'younger' or 'new' formation occurred after Ottoman conquests (Erdeljanović 1921: 67–79).

[20] Its original formulation is ascribed to K. Jireček (1959 [1879]) and Milan Šuflaj (1925). Later elaborations developed thanks to the discovery and interpretation of new archival sources – Turkish defters in the Constantinople Archive (Djurdjev 1954). Kulišić (1963: 75, 1981: 59) described the Vlach-katun theory as a social-Darwinist conception and an 'artificial', 'mechanical' construction, without grounds or confirmation in primary sources.

herdsmen katun,[21] as a form of gentile organization with military functions, was the opposite of the farming-feudal village organization.

The generators of the process of formation of tribes as forms of 'patriarchal democracy' in Montenegro (Zeta), Highlands, Herzegovina and northern Albania were to be found in the crisis of the feudal order in Raška and Zeta after the death of Emperor Dušan in 1355 (Čubrilović 1959, 1963, 1974). The 'perturbation' that spread over the whole society and turned its old strata upside down (Djurdjev 1981b: 47) resulted in anarchy and anomie. Thereby conditions were created for the formation of tribes and establishment of the military–democratic system in these territories.

The tribes, known from the Ottoman period of Montenegrin history, developed gradually from *knežine* and *vojvodstva* (dukedoms) rather than directly from Vlach katuns or feudal *nahijas* (Djurdjev 1954, 1963). At first, Vlach-herdsmen katuns were deterritorialized – they were transformed from narrow kinsmen and gentile groups into a broader territorial-type organization (descent into *župe* or districts and unification of groups of katuns); then occurred the 'stratification' of farming-village organization, carried out by the already territorialized katuns. The results of these processes were two new forms of organization, *knežine*[22] and tribes. In the tribes, the gentile system prevailed, allegedly originating from the katun organization of Vlach herdsmen.

On this interpretation, Montenegrin tribes were formed within 'Vlach *nahijas*' in the conditions of Turkish supreme control over Montenegro at the end of the fifteenth century. In the early sixteenth century Montenegro gained the status of autonomous province; *timari*, the fiefs of *spahije*, Turkish landowners were abolished, and the entire population of the *vilayet* (country) was proclaimed free peasants – *filuridžije*.[23]

In the seventeenth century Montenegro was not a 'tribal society' but rather a 'military–democratic federation of tribes and *knežine* with feudal and estate elements' and an 'ethnarchy' (Djurdjev 1974: 292–3).[24]

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[21] By that time the term 'Vlach' had lost its ethnic meaning (the Slavic assimilation of Roman Vlachs had been finished). It denoted instead a social status, the fact of belonging to the stratum of mountain herdsmen.

[22] In *knežine*, territorial organization prevailed, as demonstrated by the example of Serbia (the Smederevo sanjak), where Vlach cattle-breeders, *filuridžije,* settled there soon turned into farmers under the influence of the Ottoman 'feudal' system.

[23] The term comes from *filurija* (Tur. *filuri* – gold coin); it was the Turkish name for the tax that in the former state was intended for Vlach, herding population.

[24] The 'feudal' elements consisted of the remnants of domestic petty aristocracy and members of the church hierarchy. Their social position and political legitimacy were based on local feudal law (the continuity of tradition of the Crnojević state and of the Serbian Orthodox

Though Kulišić's solution to the problem of emergence and origin of the tribal organization was formulated in contradistinction to dominant theories, it still owes them something. Kulišić interprets the so-called re-emergence of tribes in the period of Ottoman rule as a 'renewal' of gentile forms of social structure and 'revival of archaisms', relics of pre-patriarchal gentile relations, that functionally adapted to the new context (Kulišić 1958, 1959). The key factor in the process of formation of the 'secondary' gentile–tribal organization in the Dinaric region was the symbiosis of the older Balkan (Roman-Illyrian, Vlach) and Slavic populations (Kulišić 1967a, 1980a, 1980b) rather than katun organization: the katuns did not represent a form of gentile structure, but a type of settlement (village) inhabited by cattle breeders, organized in gentes and fraternities. In this point, his conclusion does not depart significantly from the thesis of the Vlach-katun theory, which argued that the nucleus of tribal organization comes from the medieval katun organization.

In Kulišić's last analysis, the complex structure and evolution of Dinaric gentile society reflect, on one hand, the historical continuity of elements of the ancient Slav tribal organization and common law from the pre-patriarchal epoch, and on the other, the influences of the native Balkan communities.

The global solution Kulišić proposed appeared as the third one, rather peculiar, isolated and somewhat tautological. Differently from the 'logical-positivist' method of historiographers, it affirmed the dialectical approach (Kulišić 1963, 1981) which claimed to provide an insight into 'more profound and hidden realities' (see Lefevr 1973) by moving from the appearance of social forms to their 'essence' (structure) and historical genesis.

Kulišić questioned the idea that the tribal society in the Dinaric region was patriarchal by structure, historical development and origin. In the history of society 'there were no pure formations, devoid of internal contradictions'. The basic infrastructural contradiction stems from the opposition between 'old, more or less obsolete social relations, and the new ones'; 'the new that is becoming dominant and the old that is falling apart but at the same time subsisting or reappearing, in specific forms and to the extent dependent on the new social circumstances' (Kulišić 1963: 6; Godelier 1989: 203). Tribal society 'as a whole is at once lively and anachronous, and thus, in spite of all the necessary and compromise adjustments, in class societies it appears as an alien body' (Kulišić 1981: 52).

Fraternity was the main unit of the tribal society that was actually responsible for 'its basic gentile character' and the form of the gentile commu-

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Church) and on the warrants of the Turkish supreme authorities, whose representative in Montenegro until the mid seventeenth century was a Turkish landowner (spahija).

nity from which this society had developed. By its shape, structure and functions Dinaric fraternity was identical to the patrilineal, patrilocal gens or exogamous clan (Kulišić 1957a, 1957b). This is not to say that it may be identified with the primitive *gens*, but just that its 'essence' was gentile.

In reconstructing *zadruga*, Kulišić (1955b) started from Engels' and Kovalevski's conclusion that the South Slavic *zadruga* – the 'patriarchal household community ... sharing property and labour' (Engels 1979: 43) – is the transitory form and a step between the matriarchal gentile community and the modern nuclear family. According to Kulišić, the most significant indicator of such a position and the matriarchal origin of *zadruga*[25] was precisely its *fratriarchal*[26] rather then patriarchal character.

Zadruga constituted the 'economic and social essence' of such 'formations' as the Vlach katuns in the Middle Ages, and fraternities later on. By invoking the 'character of production and social relations', Kulišić (1963: 9, 1981: 64) borrowed Marx's findings in his analysis of the 'shared, immediately socialized labour' on the example of the 'patriarchal industriousness of the peasant family' (Marx 1973: 79). The household or *zadruga*-type production 'as Marx already suggested, represents the social function of family as a whole, where "individual forces act from the outset as organs of the common, family labor force"'. In this point Kulišić, probably unwittingly, approached the solution that was formulated independently by some French and American anthropologists influenced by Marxism (Meillassoux 1960, 1981; Sahlins 1972) – the idea of the so-called 'domestic mode of production'.

On the comparative–historical level, Kulišić analysed forms of social organization in the Balkans and the Caucasus,[27] reaching the conclusion that

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[25] In the structure of Dinaric and South Slavic *zadruga* Kulišić discovered 'essential elements' of a 'specific', 'transitional' and 'contradictory' form that had arisen in the stage of collapse of primitive matriarchal-gentile relations and gradual formation of the *zadruga* family with patrilocal marriage (Kulišić 1963: 9). This stage was 'frozen' in Serbian and Montenegrin wedding customs that focus on the ritual act of introducing the bride into the bridegroom's group (Kulišić 1956b).

[26] This form and 'historical type' of *zadruga*, argues Kulišić, cannot be defined as strictly patriarchal. In spite of patrilineal kinship and patrilocal marriage, it lacks the structure of the large (Indo-European) family under the despotic rule of the father or father's father. The fratriarchal character of *zadruga* stems from its egalitarian kinship structure (equality of generations and genders; democracy in decision-making and management).

[27] The novelty introduced by Kulišić in Serbian ethnology was the utilization of Caucasian data for comparative-historical purposes, probably under the influence of Mark O. Kosven, the Soviet specialist for the Caucasus and 'questions of primitive society'. Kulišić claimed to have established the identity of archaic forms in the Balkans and the Caucasus. On this basis he concluded that there must have been 'a broader and considerably older social and ethnic

these social structures formally belonged to the same type, characterized by historical longevity and a certain ability to adjust to class-based socio-economic formations (like feudalism and capitalism). These were secondary gentile communities, whose basic economic elements – restricted common production (certain forms of cooperation) and collective ownership (cf. Garodi 1967) – changed extremely slowly. Kulišić explained the persistence and/or renewal of gentile forms in the Balkans, the Caucasus and elsewhere by the similarity of historical conditions that favoured the maintenance of collective property.

With his book *O etnogenezi Crnogoraca* [*On the Ethnogenesis of Montenegrins*] (1980b) Kulišić attracted the attention of the broader and political public. The book's arguments engage in a risky interpretation of Byzantine and domestic written sources for medieval history, as well as the existing ethnographic evidence found in the works of Kulišić's predecessors. The historians, of course, intervened promptly, revealing the real content of the sources Kulišić used and the unacceptable way he used them (see Vukčević 1981).

The conclusions to this book represent Kulišić's final settling of accounts with the heritage of 'older bourgeois science', the 'national-romanticist approach' of Serbian anthropogeography and ethnology, and the 'positivist orientation' of post-war Marxist historiography and archaeology. The chief originality in Kulišić's interpretation of ethnogenesis was – unexpectedly for many – the explicitly 'heretical' statement that Montenegrins, by their ethnic origin and specific socio-historical and cultural development, are a separate South Slavic ethnicity and a modern nation. This contradicted the official view formulated by the party ideologue Milovan Djilas (1947 [1945]): that Serbs and Montenegrins have 'one and the same (Serbian) ethnic origin, one root, but their development into nations, the development of national consciousness, has taken different paths'. According to this view, until the end of the nineteenth century Montenegrins were an ethnic group within the Serbian nation, growing into a separate nation after the 1878 Berlin Congress in the process of state- and civil society-building.

Immediately after publication, the book provoked 'unprecedented turmoil', scholarly controversies and political responses. It served as the occasion for a public panel discussion *The Ethnogenesis of Montenegrins and the Marxist Definition of the Nation* organized by the Marxist Centre of the Central Committee of the League of Communists of Montenegro (1981). The topic was not new; it had been present in a highly ideologized guise since the late 1960s. The dissidents in various scientific disciplines, in rising

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substratum' underlying them, which preceded Indo-Europeans (Kulišić 1963: 56, see also 1973b, 1973c, 1974b. Cf.: Martine 1987: 90–1; Kiznije 1996: 34–5, 38).

numbers after 1968, had advocated the idea of the non-Serbian origin, 'autochthonousness' of the Montenegrin people and the 'sovereignty' of the Montenegrin nation. Kulišić's book, which in specialist terms probably did not deserve the attention it was accorded, was treated as the expression of the revival of national 'romanticism' in domestic science (Djurdjev 1981b: 46), encouraged by the political aspirations for Montenegrin sovereignty and secession. It was instrumentalized in the politicization of ethnicity and, at a critical moment, it functioned as the 'litmus test of being for or against Montenegrin nationhood' (Vujović 1981: 57; Brković 1988).

In social reality, however, the scientific pertinence of a discourse says nothing of its ideological relevance for the members of a given group. From the perspective of independence Montenegro achieved by the May 2006 referendum, one could argue that Kulišić's ethnogenetic discourse, regardless of his intentions – today we can only speculate about them anyway – played a part in such an outcome and generally in today's identity politics.

Conclusion

The characteristic motifs in the narrative about our author – the posts and roles he took in his relatively brief active career (a successful museum official, ideologist and hated polemicist in the profession), his professional mobility and the paradoxical turn he made in his advanced age – reflect the specific conditions and main trends in the dynamic of Yugoslav society. From a pretentious ideologue of supra-national ethnology of the socialist society – where the national question was allegedly 'resolved most consistently' by applying the principles of Marxism–Leninism (the platitude of brotherhood and unity of all Yugoslav peoples and nationalities) – he turned into an advocate of Montenegrin nationalism, that is, of the political programme that many years later would bring to its end the process of Yugoslav disintegration, by breaking up its last 'relic', the federation of Serbia and Montenegro. From an 'ideologically–politically' conscious ethnologist of Marxist persuasion, who was always on the alert against signs of 'metaphysical thought' and ideologically unsuitable paradigms in science, he became a doctrinaire advocate of unilinear evolutionism. He never realized that his pseudo-historical reconstructions and, particularly, the discourse on the origin and development of the tribal society in the territory of Montenegro and Herzegovina, anecdotally labelled 'evolutionary structuralism', suffer from the same shortcomings that he used to ascribe to others – atemporality, schematicism and national romanticism.

What does this individual case tell us about ethnology in the socialist period? In spite of ideological imperatives that called for a break with the past, in the new theoretical framework we discover the existence of signifi-

cant continuities with the pre-war tradition of the discipline – in terms of the definition of the science's subject and character; classical topics and grand questions; and methodological accent – the comparative–historical and cultural–genetic method.

The contribution of historical materialism in innovating the traditional paradigm may be viewed from different aspects. At the theoretical–methodological level, its positive sides are: a consistent, though schematic and too extensively applied comparativism, at a time when it had all but disappeared from sight in domestic ethnology; the potentially fruitful approach to traditional society and culture in terms of an infrastructure–superstructure dialectic developed in the early 1960s – an approach that mostly remained an unfulfilled promise, since it was premature and theoretically ill prepared in comparison with the subsequent development of Marxism and structural Marxism in anthropology. The basic evolutionary approach and the partial solutions – reconstructions of kinship, ethnic history and, particularly, folk religion, mythology and customs – that Kulišić proposed under the label of Marxism/historical materialism, won a number of adherents who continued working in a similar style (e.g. Kostić 1969; Pantelić 1980, 1981; Petrović 2000a, 2000b, 2002), disregarding the theoretical sign or without any explicit theory altogether.

As far as the institutional aspect of scholarly research is concerned, we can identify the innovations that may be counted among the benefits of the socialist heritage, such as: collective projects and teamwork, field research and complex, sometimes interdisciplinary studies of regional wholes, ethnic and social groups; publication of results (collective monographs, edited volumes) and systematic museological activity.

Publishing, debates and criticism in the academic community, though infrequent and contaminated by ideological and personal interests, demonstrate that historical materialism was not the only, not even the dominant paradigm, and this also holds for the years of the Cold War and estrangement from the Soviet Union. At that time already there existed an alternative strategy that opposed antiquarian selection of topics and conjectural history by the study of contemporary times and the application of ethnological knowledge in social practice. The critique of 'Špirinism', at first implicit, and then increasingly overt, though politically undesirable and potentially subversive, could be tolerated when it was voiced in the name of a more authentic, classical and humanistic Marxism.

The negative assessment of historical materialism within Serbian ethnology was a logical outcome of Kulišić's failure to offer the discipline a more persuasive demonstration of the potentials of Marxist analytical categories. Further reasons include animosity to Marxism as a consequence of

ideological saturation, and indifference to anthropological theory in general until the 1970s. For these and other reasons (such as a shift of interest from the traditional and rural to the contemporary and urban), the classical issues of national ethnology and historiography, which in socialism not only survived but were reworked within the historical–materialist paradigm, are nowadays largely neglected and abandoned, although many of the problems involved have remained unsolved.

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Chapter 15
The Art of Mutual Neglect: Zagorka Golubović's Socio-cultural Anthropology and Serbian Ethnology

Ivana Spasić

Apart from institutionalized ethnology, in Serbia there also existed another conception of the academic study of man and culture: 'socio-cultural anthropology' personified, for the most part, by Professor Zagorka Golubović. With remarkable consistency across the almost fifty years of her career, Golubović has in her writings and teaching argued for a specific vision of anthropology. The social history of this vision, both in its internal, theoretical aspects as well as in the external, political and institutional ones, is representative of the peculiarities of Yugoslav socialism and the ways it shaped the academic study of culture and society. All the dilemmas and strains of the Yugoslav 'self-management' socialism, of its theoretical legitimation and its practice of ruling, are mirrored, as it were, in the story of Zagorka Golubović's socio-cultural anthropology.

The Nature and Scope of 'Socio-cultural Anthropology'

Born in 1930, Golubović grew up in a middle-class family of leftist political persuasion.[1] After an *engagé* adolescence – she was an activist in the communist youth organization – she studied philosophy, graduating in 1952. During her graduate studies, she geared her interests into social philosophy, though her first academic appointment was Teaching Assistant in the History of Philosophy (1957). As a grantee of the British Council, she spent the 1957/58 academic year at the University of Nottingham. This visit to Britain would prove decisive for her final reorientation to anthropology. It was on the basis of the ideas and literature collected in Nottingham that she chose her Ph.D. topic – 'Problems of Contemporary Theories of Personality' – and completed her dissertation, obtaining the degree in 1962. Appointed Assis-

[1] In an autobiographical sketch (2001), Golubović recalls that a strong feeling of 'social justice' was something she imbibed in her family environment and made part of her own mental and emotional makeup very early on.

tant Professor at the newly founded Chair in Sociology within the Department of Philosophy, she was not assigned any of the already existing courses. Instead, she created a completely novel course, Socio-Cultural Anthropology, which was approved in 1963 and introduced into the Sociology curriculum.

It was no accident that Golubović's initiative was accepted at just that moment. The early 1960s were a period of intense agitation in the political and intellectual life of Yugoslav society. After its break with Stalin in 1948, Tito's Yugoslavia searched for a new identity. Without in any way renouncing socialism, the ascendancy of the Communist Party, or Marxism–Leninism as the basic ideological framework, Yugoslavia did reject Soviet models of institutional arrangements, seeking to legitimate itself as a country 'neither East nor West', socialist but not Stalinist, run by communists but obedient to no one, including the Soviet Union. The ideology of 'socialist self-management' was devised and developed from 1950 on, with precisely this purpose in mind – to devise a completely novel socio-politico-economic system that would be, it was claimed, more true to the original ideals of Marx, Engels and Lenin.

The critique of Stalinism necessitated by the desire to distance the Yugoslav regime from the Soviet Union and the Eastern bloc implied new openings for artistic and intellectual freedom, though within the confines of the officially prescribed world-view. The late 1950s were marked by several important events: the 7th Communist Party Congress in 1958 when a new, comparatively liberal Party Programme was adopted; and the Congress of Writers and Philosophers held at Bled in 1960, when a crucial clash between the party 'liberals' and 'hard-liners' took place, to the benefit of the former. The founding of the first Chair in Sociology, at the Faculty of Philosophy in Belgrade, in 1959, was part of this climate. This was one more proof that Yugoslavia was different from other real-socialist countries, since in most of them sociology was officially proscribed and replaced by 'historical materialism'.[2] This was precisely the context for the introduction of the Anthropology course. The latter had to overcome some resistance, triggered by the fact that Golubović's course materials abounded with 'unnecessary' Western ideas.[3]

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[2] It is generally believed that the admission of sociology was motivated by the regime's desire to bolster its legitimacy in the new, post-1948 circumstances. The scope of professional liberties for sociologists – and, we may add, other students of culture and society – were closely conditioned by the changing relations of forces among different factions within the Communist Party (Vuletić 1996: 9).

[3] Source: interview with Zagorka Golubović conducted on 24 May 2006. In the text, this source will be referred to as *Int.*

At the same time, the first Yugoslav sociological journal, *Sociologija*, was launched (1959), with a telling subtitle: *Journal of Sociology, Social Psychology and Social Anthropology*, and Rudi Supek as editor-in-chief.[4]

The next important step in institutionalization was taken in 1974 when, again upon Golubović's initiative, the Graduate Interdisciplinary Chair in Anthropology was established at the Faculty of Philosophy. An inter-departmental body, the Chair acted as a meeting-place for teachers from different sections of the Faculty, bringing together specialists in philosophy, social sciences, arts and humanities who offered graduate-level courses within their respective spheres of competence.[5] After Golubović's short-lived leadership, the Chair's longtime chairman was Dragoslav Srejović, an internationally renowned archaeologist. Over the years, the Chair included members such as Živko Mikić, archaeology; Zdravko Kučinar, philosophy; Bora Kuzmanović, social psychology; several ethnologists (Đurđica Petrović, Petar Vlahović, later on Branko Ćupurdija and Mirjana Prošić-Dvornić); and a host of sociologists, as the links to this Department have traditionally been the strongest (Anđelka Milić, Mladen Lazić, Sreten Vujović, Marija Bogdanović etc.). In the period 1974–90, the Chair accorded twenty-four M.A. degrees and several Ph.Ds.

Socio-cultural anthropology, as the core of this interdisciplinary endeavour, was personified by Zagorka Golubović. She did not think that the work of any Serbian ethnologists really converged with her vision of it. Instead, she found like-minded colleagues primarily in Zagreb, where she also taught several times as visiting professor, most closely cooperating with the social psychologist–philosopher Rudi Supek and the ethnologists Vera Erlich and Dunja Rithman-Auguštin.

From her very first writings (1966, 1967) to the most recent (1997, 2005) Golubović has remained faithful to a definition of anthropology that sets the latter apart from all other disciplines, including ethnology. Let us review some of the building blocks of this understanding.

(1) *Man as the subject matter*. Firstly, the meaning of the label 'anthropology' is explained etymologically: as a combination of *anthropos* and

[4] How much the label 'anthropology' was implicated in intellectual–political battles is indicated by Supek's recollection that this subtitle was dropped in 1961 'under the pressure of the dogmatics who found it disturbing'. The subtitle's reappearance in 1963 was a victory for 'the people who wanted the social sciences to develop in the direction of a humanistic and anthropological conception of Marxism' (Supek, quoted in Bogdanović *et al*. 1990: 184).

[5] This is what Golubović says nowadays: 'The Faculty of Philosophy was in an ideal position to connect disciplines which are really interconnected: philosophy, sociology, psychology, history, art history. Unfortunately, all this remained compartmentalized ... I wanted to establish links among all these, at least at the graduate study level' (*Int.*).

logos it is, Golubović claims, 'literally "the science of man"' (*Int.*).[6] When it is social and cultural, it focuses on 'man as the being endowed with the capability to create material and spiritual values and thereby to transform nature into a human world' (1967: 18). It is a systematic, historically grounded, trans-cultural discipline that, 'studying man in different social communities', aims at 'establishing how man has developed historically through his praxis and how, creating culture, he humanized his environment, as well as how in a particular socio-cultural setting he is constituted as a human person' (1967: 35). Anthropology thus 'collects arguments to explain the essence of human living' (1967: 19).

(2) *Human nature*. In order to achieve this aim, anthropology must necessarily posit *human nature* as its starting point. 'Without the notion of the "human nature" anthropology could not constitute itself as a science' (1997: 41).[7] This entity called 'human nature' may be arrived at by identifying universal elements across cultures. They are at the same time what make man different from all other living beings. Following closely Marx's notion of 'species being', Golubović's conception of human nature is fundamentally based on *praxis* and includes such elements as purposeful and conscious action, symbolization and creativity, sociality, and, most importantly, ability to transform natural and man-made environment – to 'transcend the given'. In sum, 'human essence is a reservoir of infinite possibilities' (1973: 39).

(3) *Normativism*. Human nature also involves a notion of gradation, of an evaluative hierarchy of manifestations of human potentials. For, humanity may be realized to a lower or higher degree, in less or more perfect forms. It is a central feature of the human 'essence' that it exists both in the mode of imperfect actuality, and in the mode of ideal continuously to be approximated. Potentials are an open category, so that man is capable both of production and destruction, perfection and decline, improvement and regression. Thus anthropology must also study 'failures and alienation of man, what makes him less human' (1967: 35). Anthropology is both able and obliged to distinguish between positive and negative sides of human development, to identify distortions, and to point at how these could be rectified.[8]

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[6] In this text, the Serbian term *čovek* will be translated as 'man', both for the sake of simplicity and in order to preserve the 'politically incorrect' connotations of the original. For, in Serbian, *čovek* – quite similarly to the English 'man' – involves a play between neutrality (human) and gender markedness (masculine).

[7] Golubović continues: '…and this is the difference between anthropology and ethnology, since the latter studies peoples and ethnic groups, and cannot make generalizations referring to the human species' (*ibid.*).

[8] Normativism is intimately related to the notion of a universal 'human nature', since 'if anthropology did not seek to uncover what is universal … we would not be capable of

(4) *Totality*. Totality is understood as a twin characteristic of both humanity and the science studying it. Man is a 'total phenomenon' in the sense that he is a being at once biophysical and socio-cultural, rational and emotional, individually unique and socially shaped, and finally capable of either positive or negative development (freedom and constraint, creativity and conformism, self-actualization and destruction, love and aggression etc.). Similarly, the discipline of anthropology is equally 'total' in that it is at once philosophical and scientific, abstract and concrete, existential and sociological, subjective and objective; it studies 'all aspects of human existence, in all historical periods' or 'the mode of human existence' (1994: 226).

(5) *Interdisciplinarity*. In close connection to the preceding point, anthropology is fundamentally an interdisciplinary undertaking. It combines natural and social sciences and, within the latter, various disciplines: social and cultural anthropology, ethnology, psychology, social psychology, sociology, history, archaeology etc.[9]

(6) *Philosophizing*. With such a specific subject matter, anthropology cannot be a science like any other: it is strongly philosophically informed. It shares its tasks with humanist philosophy, since it treats 'the basic questions about man' (1967: 18), or 'existential issues of humanity' that are 'always in a way "metaphysical"' (1966: 6).[10] The difference is that anthropology translates these speculative questions into the language of scientific research, but there is no necessary clash between the two.

(7) *Focus on individuality and personhood*. Finally, unlike any other social science, especially sociology, anthropology must transcend the typical and the average, to make room for what is individually unique. 'The distinctive trait of anthropology is the relation of *man*, and *individual* man at that, and culture' (*Int*). Anthropology in general is seen as a symptom, and simultaneously an agent, of the rupture with sterile positivism in social science, since 'the human phenomenon … all that belongs to man's internal world, his subjectivity, cannot be "measured" … This would entail the sacrifice of

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drawing the line between human and inhuman, and thus could not relate critically to the latter; for, the task of anthropology … is not just to establish the facts … but also to ascertain in which forms broader possibilities for the realization of man as a human being are manifested, and where these possibilities are considerably reduced, and why' (1973: 17). This vision, she stresses, 'differs fundamentally from the view currently prevalent in British social anthropology and American cultural anthropology' (*ibid*.).

[9] We can see how the creation of the Interdisciplinary Chair meant bringing to life a major theoretical commitment.

[10] A list of these 'basic questions' involve: 'What is man; what does the specific character of the human nature consist of; can lawful regularities in the development of man be established; what are fundamental human needs; what are the consequences of man's alienation?' (1967: 18).

precisely what we want to uncover – human distinctiveness, individual experience' (1967: 6).

This vision of socio-cultural anthropology had a clearly identifiable genealogy. Its main theoretical inspirations could already be discerned from the definitional elements listed above. The single most important theoretical source was humanistic Marxism. In the early years, Golubović defined her standpoint unequivocally as a 'Marxist anthropology'.[11] This was the time when Marx's 'Early Papers' were being rediscovered, provoking a huge movement of reinterpreting Marx away from economic determinism and Stalinist petrification. Within the circles of the Western New Left, as well as among the less dogmatic thinkers in Eastern Europe, concepts such as praxis, labour, alienation, reification, freedom, self-actualization, creativity were gaining ground, providing a new common language to critique the distortions of societal development in capitalism and state socialism alike.

This was the primary theoretical environment in which Golubović's socio-cultural anthropology was designed. In addition to sharing the basic categories with humanistic Marxism, it also shared its built-in ethical stance, noted above as 'normativism'. This stance rests on the assumptions that there *are* recognizable human values that should be upheld, that a better society *is* both imaginable and feasible, that social and psychological conditions *can* be evaluated as either hindering or facilitating human and humane development. This entailed the legitimacy of making evaluative distinctions within empirically available data between what is 'good' and what is 'bad'.

The second main source was the tradition of social and cultural anthropology, both American and British, especially the 'culture and personality school'.[12] Then there were the more strictly philosophical sources like Sartre's existentialism and the personalism of Emmanuel Mounier. References to German philosophical anthropology (A. Gehlen, H. Plessner) were also prominent. Psychology was introduced via the works of G. Allport, J. Piaget, and especially Erich Fromm, probably the author Golubović found most congenial.[13]

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[11] She described all three major works of her early period (1966, 1967, 1973) as a unified whole, representing 'an attempt at developing the Marxist standpoint in anthropology' (1973: 3).

[12] 'Ruth Benedict and Margaret Mead strongly influenced my orientation, since they placed human personality at the centre' *Int.*). Golubović met Margaret Mead in person, at an anthropological conference in Moscow in the early 1960s.

[13] Apart from using Fromm amply in her writings, Golubović also contributed to an early acquaintance of the Yugoslav intellectual public with Fromm's works as the editor of publishing series and scholarly journals, where she initiated the translation of a host of his works during the 1960s and 1970s.

Empirical investigations never had a very precise place in Golubović's anthropological undertaking. Due to its strong philosophical bent, it never posed fieldwork in the traditional form of ethnography as an imperative. She did venture into empirical work, though, firstly more in the ethnological vein, when she directed a study of changing values in peasant families in Serbia in the early 1970s. Later on, she based a major book (Golubović 1988) on data derived from a mixture of sources, combining secondary analysis of official statistics and sociological surveys, observation, and structured interviews with members of the old generation of Yugoslav communist leaders. The rest of her works belong more to the genre of 'cultural critique', being based on unsystematic observation of the empirical world, especially the contemporary Serbian/Yugoslav society, and on critical readings of other people's writings. This unclear status of empirical grounding is interesting, because it tells us a lot about the nature of this version of sociocultural anthropology. On one hand, its relation to empirical data is flexible, since it certainly does not rule out empirical references, but it also does not stick to any established methodology. It is guided by *an idea* rather than by some strictly defined methodological procedures. One could moreover argue that strict adherence to any particular methodology would within this framework be undesirable, since it would entail an artificial narrowing of horizons. On the other hand, this lack of commitment to fieldwork may just as well be seen as problematic, as the source of many of the weaknesses of Golubović's anthropology, especially in conjunction with its normativism.

In defining the relation of 'her' anthropology to ethnology, Golubović has consistently referred to Levi-Strauss' threefold and hierarchical division ethnography – ethnology – anthropology. In spite of the partially shared ground, ethnology and anthropology are clearly distinct, separate and in several respects mutually exclusive disciplines; they may cooperate and complement each other, but never merge into one.[14]

Her view of the relations between ethnology and anthropology may be presented through a series of binary oppositions:

(a) descriptive vs. theoretical and conceptual
(b) analytic vs. synthetic
(c) particularistic (ethnically bounded) vs. universalistic[15]
(d) objective, structural and macro vs. focusing on the personal and the subjective[16]

[14] She strongly opposed ethnology's strivings, especially in the late 1980s, to broaden itself into 'ethno-anthropology'. The polemic taking place in the early 1990s (Pavković 1992; Golubović 1994) revived some old motifs of this debate and added new ones.

[15] Hence, says Golubović, anthropology cannot be defined as a 'science of the Other' (1994: 226).

(e) exclusively a social science vs. interdisciplinary, natural + social science.

Political Vicissitudes

The above discussion of the institutionalization of Golubović's vision of anthropology presented its successful side. However, its trajectory in post-war Yugoslav society also had its gloomier side. This other side results from the fact that the whole 'socio-cultural anthropology' project was an integral part of the broad intellectual movement most often denoted by its general label 'the *Praxis* philosophy'. Sharing its basic theoretical premises, Golubović's anthropology, and Golubović personally, also shared the social and political fate of this movement.

The *Praxis* circle was a group of Marxist scholars, concentrated mainly in faculties of philosophy in Zagreb and Belgrade. The local branch and brand of humanistic Marxism, they applied innovatively the ideas of 'young Marx' to various abstract and concrete issues, including a Marxist critique of the Yugoslav society of the period. The main outlets of the group were the *Praxis* journal (1964–74) and the Korčula Summer School (1963–74). Both the journal and the summer-school sessions involved not just local scholars but prominent leftist intellectuals from West and East, including such intellectual stars of the times as L. Kołakowski, Z. Bauman, A. Heller, G. Markus, E. Bloch, H. Lefebvre, H. Marcuse, J. Habermas, A. Wellmer, R. Bernstein, or A. Touraine.

The relation of the *Praxis* group to the Yugoslav regime was ambivalent and paradoxical. On the one hand, both at the ideological and at practical levels the group was intimately tied to the official ideology. In terms of ideology, the official rhetoric and the *Praxis* discourse had a significant part of their preferred vocabularies in common, starting with the commitment to a 'socialism with a human face', to the particular categories such as 'self-management', 'socialist freedom', 'alienation' and 'disalienation', critique of 'petty-bourgeois values' etc. From the present-day perspective, there are grounds to argue, as the more cynical critics of *Praxis* are fond of putting it, that *Praxis* members were no dissidents at all, since they were 'whiter than white' – even more puritanically Marxist and more anti-liberal than the regime itself. At the practical level, the members of the *Praxis* group virtually dominated university departments of philosophy and sociology in the

[16] 'Ethnology, in the earlier period when it was simply ethnography, as well as in the current period, has not been personalized, while in anthropology the personalized status of the human being is an essential characteristic. And this is precisely what forced my shift from sociology to anthropology, since sociology was at the time a highly institutional discipline' (*Int.*).

1960s and the early 1970s; in this light it may be argued that this was *the* hegemonic social philosophy of the epoch.[17] The institutionalization of sociology, and Golubović's anthropology under its wing, could not have been such a success if some of the leading *Praxis* members had not had a powerful negotiating position and a privileged access to the centres of political decision-making (Veljko Korać, a distinguished member of *Praxis* and the 'dean' of philosophy at the University of Belgrade, is thought to have been the chief mediator between the intellectual and the political circles).[18] Golubović herself, a prominent member of *Praxis*, served a two-year term as a MP in the Federal Assembly (1962–3), and travelled to New York to attend the UN General Assembly as a member of the official Yugoslav delegation headed by Tito personally.

On the other hand though, the relation of *Praxis* to the regime was by no means only idyllic. Confident in their Marxist legitimization and backed by their international network of prominent friends, *Praxis* members never shunned denouncing the practices of the regime, its leading figures, and its 'never-really-fulfilled' promises of self-management. This critique was firmly grounded in the classics of Marxism and, indeed, definitely 'communist'; but this should not blind us to the fact that it demonstrated a distinct lack of awe and respect for powers that be, a courage that was quite rare for those times, even in Yugoslavia. The regime tolerated their activity, partly because of the high political standing that some of the members enjoyed, and partly because this tolerance supplied it just the right kind of legitimacy it needed in the eyes of the Western public. However, the relations grew more tense after June 1968, when *Praxis* members actively participated, or more precisely led, the student-faculty protest, the Yugoslav reflex of the world-wide 1968 movement. After that, the repression increased, assuming various forms – from banning the journal and the Korčula Summer School, to denunciations in the party bodies and the press, to court trials.

Peculiar in many ways, Titoist socialism was peculiar also in its mechanisms of exerting political control. It generally did not assassinate dissidents, send them to gulags, or even imprison them – though there are important exceptions. Instead, the regime used the 'softer' forms of, let us call it, delegated repression, whereby upper agencies of power devolved to their confidants within the professional milieus (universities, editorial

[17] How much this differed from the situation in Eastern Bloc countries may be illustrated by a 1968 episode: when Zygmunt Bauman and Leszek Kołakowski were thrown out of Warsaw University, the Faculty of Philosophy in Belgrade issued an open invitation for them to join the ranks of its professors.

[18] When the political decision to open Sociology studies was made, the faculties of Law and Philosophy vied for hosting them. Philosophy won, presumably thanks to Veljko Korać.

boards, professional organizations etc.) the tasks of removing undesirable people from their posts and 'unfitting' ideas from the public view.

Some outstanding instances of such repression involved Golubović personally. In 1973, her newly published *magnum opus, Man and His World in an Anthropological Perspective*, was put to trial – the book, not the author – and the ruling allowed the book to return to the bookstores, but purged of its chapter containing a critique of Yugoslav society. This criticism, characteristically for the entire *Praxis* position, was two pronged: on one hand, it targeted 'capitalist' elements, such as the growing importance of the market and the attendant consumerism, mass culture and increasing social inequalities;[19] on the other hand, it exposed the Stalinist derived 'deviations' from the supposed truly Marxist conception of socialism, like the usurpation of power by political leaders, stymieing of the spontaneous workers' self-organizing, and repression of free cultural creativity. Not surprisingly, the so-called 'bureaucracy', a separate stratum which, monopolizing decision-making and enjoying inordinate privileges, embodied at once both sorts of social ills, constituted a favourite object of censure (e.g. 1973: 510–19). Whatever the direction of critique, anti-capitalist or anti-Stalinist, the humanist Marxist notion of human nature, with all its needs and propensities outlined above, served as the paragon, since both systems were guilty of perpetuating alienation and thus pushing humankind farther and farther away from the path of genuine emancipation. The same ideological–theoretical model was closely followed in Golubović's later critiques of 'actually existing socialism' – its Soviet version (1982),[20] and again the Yugoslav one (1988). The difference is that the tone got distinctly gloomier as time passed. In 1973, she could still talk about 'a new historical chance' lying in the unrealized potentials of Yugoslav self-management (1973: 525); in 1988, the analysis sounds rather as a diagnosis of terminal illness. Though the foregrounding of the gap between 'ideals' and 'reality' was central to Golubović's critique of socialism in general, the gap was ironically wider in the Yugoslav case. The reason was that the Yugoslav self-management experiment had aroused ardent hopes that it would prove that it had *finally found* a model of a society that would be workable and at the same time favourable to the free expression of human nature. In the censored part of *Man and His World* (1973: 492–525), this play on 'ideals vs. reality' is quite overt: Golubović opens the chapter with a longish quote from the Yugoslav Consti-

[19] In a sharply critical mode, she presents a detailed analysis of differentiation among social strata in Yugoslavia along various dimensions – material status, political power and participation, privileges, access to culture etc. (1973: 502–10).

[20] In this study she argues that Stalinism, having betrayed all fundamental socialist values, was not socialist at all, and that the Soviet system never really superseded its Stalinist legacy.

tution, and continues by tracing point by point all the constitutionally guaranteed rights and values that were trampled in practice.

The second occasion when Golubović was struck by the regime's hand occurred in January 1975, when she found herself among the eight distinguished professors of philosophy and sociology who were removed from university teaching for political reasons. This was actually a deferred consequence of their involvement in the June 1968 events. The decision to suppress the 'culprits' had been made at a closed meeting of Tito with the Serbian communist leadership in 1972 (Popov 1989: 35). Its implementation took so long because this time the 'soft' mechanisms did not work properly – the Faculty bodies resisted dictates from above for years, using all available 'self-management' means. In the end, a special law was passed in order to provide legal cover for their removal. They were placed 'at the disposal' of the authorities, officially retaining their posts but forbidden to teach, since the main concern of the regime was to prevent their contact with the students. In 1981, a special research centre – the Centre for Philosophy and Social Theory within the Institute of Social Sciences – was formed, where *Praxis* members continued to work and write. Socio-cultural Anthropology as a course was at first removed from the curriculum, and then reappeared as an elective, taught by another teacher.[21]

The third episode took place in the 1980s. The dissident group formed around the *Praxis* core continued writing, but as they were banned from public appearances they used private spaces for holding meetings. In 1980, it was on the initiative of Golubović, recently returned from a visit to Poland in the midst of the 'Solidarność' turmoil, that the 'Free University' was established, modelled after the Polish Flying University. It was a series of lectures involving scholars and students from various disciplines, held in private homes, most often in Golubović's own. Though informal, the meetings were structured: topics and speakers were planned in advance, and a debate followed. Attendance was open. The secret police monitored the meetings through their agents, but refrained from interfering directly. The leaders, including Golubović, were called to the police premises several times for 'informative briefings', but no one was arrested until 1984, when in April a police raid interrupted one of the meetings and detained twenty-eight participants. Later that year, the 'trial of the six', one of the more famous political trials in Yugoslavia, was held and some (student) participants ended up in jail.

[21] Ironically, this very same teacher had been among the very few members of the Department who at a critical moment sided against the *Praxis* group. Golubović's permission was not sought for reactivating the course; she did not protest, though obviously she was not happy about this coincidence.

The period until 1990, when she returned to the University together with her colleagues, Golubović spent writing and teaching at foreign universities (in Sweden, Great Britain and the USA).[22]

Thus, socio-cultural anthropology was cut short in its development, prevented from spreading and being transmitted to the younger generations, by a political decision. It seems however that the regime did not find anthropology *as such* politically undesirable. The party never meddled with the contents of the course while it was taught.[23] What disturbed the regime was the criticism of the Yugoslav reality. This is what prompted the suppression of the *Praxis* movement, rather than the theoretical contents of their writings and courses.[24] Yet, Golubović's vision of anthropology included as its integral part a critical, politically committed aspect of applying theories to the actual conditions in one's social setting, in order to contribute to its improvement in the direction of a 'more humane' (socialist) society. Viewed from this angle, Golubović's socio-cultural anthropology *was* at odds with the regime, and the conflict was inevitable.

Conclusions

So, what does this story of 'socio-cultural anthropology' tell us today of the possibilities opened up for studying culture and society under socialism, and in particular of the setting in which social sciences worked in the Titoist Yugoslavia?

There are two aspects that ought to be discussed. Let us start from the more political one. What should be noted at the outset is that 'socio-cultural anthropology' *was possible* at all. The unique combination of openings and constraints that characterized the Yugoslav regime enabled Zagorka Golubović to introduce fresh ideas from the West and synthesize them with other sources into her particular conception. But then, this conception finally came under attack and was suppressed politically. The third twist of the argument is that this conception could not have been possible, even in Yugo-

[22] As Golubović recalls, her Western colleagues were surprised that she, as a dissident, was allowed to travel abroad. She used to explain: 'They don't want me to poison our students, but are happy to send me over here, to poison your students' (*Int.*).

[23] An anecdote further illustrates the point. At a congress in Moscow, Golubović had the following exchange with the official Soviet escort–supervisor of the Yugoslav delegation, a woman ethnologist: 'I don't agree with any of what you have said. Has your state approved of your presentation?', asked the Russian. 'My state does not even know I am here', Golubović replied (*Int.*).

[24] Golubović shares this opinion (*Int.*).

slavia, if it had not been based on Marxism.[25] When the current critics of *Praxis* disdainfully say the whole fuss about their political persecution was just a 'quarrel in the family', there is a grain of truth in that. But it is even more certain that one must resist the temptation to write off the whole *Praxis* endeavour – and 'socio-cultural anthropology' within it – as insignificant, failing to realize how much this limited pluralism provided real gains for the development of academic social sciences and allowed for a lively intellectual life. The breadth of Western literature Golubović used in her works, along with the ideas of East European scholars, were undeniably extremely significant in terms of offering other professionals and students substantial new horizons for reflection and research.

The second point refers to the treatment of ethnology. Golubović's view of ethnology presented above may be said to be rather selective and, sometimes, plainly unjust.[26] The problem seems to be that she kept to overly rigid vocabulary definitions of the two disciplines, without paying attention in any detail to what real, living Serbian/Yugoslav ethnologists were actually doing, where much more convergence could be found.

Golubović herself and her Interdisciplinary Chair entertained uneasy relations with the Department of Ethnology at the same Faculty of Philosophy in Belgrade. While short of an actual war, these relations can be described as full of subdued tension.[27] Both sides saw each another as competitors rather than partners; and both strove to keep the two institutional nodes apart. There was no official cooperation, especially not on any common projects. Yet, there were no overt conflicts, no scandals or bloodletting that so often characterize academic rivalries. Apart from sporadic and parenthetical skirmishes (e.g. Rakić 1970), the two disciplines for the most part lived in the state of blissful mutual neglect.

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[25] Golubović, like other *Praxis* members, is not even today aware of this routine and tacit censorship accomplished by *a priori* exclusion of any non-Marxist thought, e.g. when she says that her 'writings and teaching were completely free of any ideology or censorship' (*Int.*). *Praxis* played the role of 'intellectual opposition' in a system whose lack of institutionalized political opposition they not only failed to criticize, but rather actively supported.

[26] E.g. when she insists on its 'merely descriptive nature', on its strict ethnic boundedness and lack of comparison, on its blindness to personhood and individual subjectivity. Today still, she says: 'The reason why anthropology here, at the moment of its founding, did not have any closer links with ethnology, is that the latter was pure ethnography ... It is only recently that it is becoming ethno*logy*, in the precise sense of the term' (*Int.*).

[27] Their ambivalent nature is illustrated by the way Golubović speaks about them nowadays: she cites several names from Ethnology Department as significantly contributing to the work of Interdisciplinary Chair, while at the same time claiming that it was from the very same Department that the strongest resistance and most of the obstacles the Chair encountered originated (*Int.*).

As a final note, there is a paradoxical coincidence between some features of Golubović's vision of socio-cultural anthropology and some features of what she opposed most fiercely, namely ethnology self-understood as a 'national science', depicted by Naumović (2005: 24–6).[28] The notions of the 'national character' and of the 'human nature' occupy structurally equivalent positions within the two conceptions. Both are characterized by what may be termed *positivity* and *normativity*. Positivity is contained in the view that there is a conclusive list of features that make up these entities – national character and human nature, respectively – defining them unequivocally and setting them apart from other similar entities, with the corollary that science can help to compose this list. Normativity postulates that science is obliged to place its scientific capabilities in the service of a non-scientific 'good': as the said entities are in a less-than-perfect condition, science can and ought to assist in streamlining them in accordance with the ideal.

As an intellectual conception, Golubović's socio-cultural anthropology bore all the virtues and vices of *Praxis* discourse. Among the vices, the weightiest ones were normativism and a lax attitude to empirical grounding, what Savić (2004: 208) calls 'the dangerous negligence to the complexity of social facts'. Viewing ethnology as its inferior 'younger sister', this anthropology consistently kept aloof from the latter. Thus the chance was forgone to jointly construct a new, comprehensive and dialogic discipline of studying culture and society.

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[28] As Naumović says, 'national' ethnology was expected, among other things, (1) to 'affirm that there is a recognizable, distinct and positive identity of one's own nation or ethnic group', and (2) to 'enable the improvement of [its actual] condition, i.e. eradication of those deficiencies that lead the community the wrong way' (2005: 25, 26).

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Chapter 16
Serbian Village Culture: The Meeting Point of American Anthropology and Serbian Ethnology

Mirjana Prošić-Dvornić

Beginnings

Serbian ethnology was founded in the late nineteenth and early twentieth century on three different paradigms: folklore as defined by the philologist and folklorist Vuk Karadžić, common-law research by the legal scholar Valtazar Bogišić and the anthropogeographic school developed by the geographer Jovan Cvijić. These were all rooted in Romanticism and instrumental in nation and state building (revised alphabet, standardized language, canon of national traditions), delineating ethnic boundaries (spatial distribution of cultural elements, migrations), and determining ethnic/folk specifics (type of social organization, customs and other religious practices). The formation of ethnology and its rise to the status of prestigious 'national science', together with history, art history, archaeology and linguistics, coincided with Serbia's political liberation and aspirations of uniting all Serbs who had, due to historical circumstance, lived under different foreign domains (Ottoman, Habsburg and Venetian), within a single state (the 'Greater Serbia' concept). From that standpoint, Herder-like ethnology, ready to delve into the past of its own people (the peasant majority) and determined to discover the true *Volksgeist* was a perfect fit for the goals of Serbian politics striving to restore its medieval glory and seek revenge for loosing its freedom and power to the Ottomans, as epitomized by the Legend of Kosovo.

For these reasons, as in other part of Eastern and Central Europe, faced with similar challenges, this particular 'brand' of ethnology was chosen over an alternative path, an ethnology grounded in the philosophical premises of the Enlightenment. A possibility for this other option, an ethnology/anthropology in the spirit of positivism looking outward, towards the world, and seeking comparisons, generalizations and laws, was offered by the Serbian educated elite in Austria, notably by one of the greatest Serbian scholars of the Enlightenment, Dositej Obradović, his predecessors and followers whose work taken together spans the period from the mid-

eighteenth to the mid-nineteenth century (cf. Kovačević 2001). However, national aspirations and international circumstances surrounding the decline of the Ottoman empire and bringing into play conflicting interests of great powers attempting to resolve the 'Eastern Question', influenced the decisive turn in the mid-nineteenth century from the great European tradition of liberalism, cosmopolitism and belief in the strength of reason to the other, based on ethnic nationalism and historicism.

The Absence of Development

If the 'national science' paradigm was the only feasible alternative while the nation was making every effort to attain its objective, did it not become obsolete once a version of the main goal, the national unity, had been accomplished? Namely, after World War I (Jovan Cvijić participated at the Versailles Peace Conference as an expert on boarder delineation) the majority of Serbs found themselves living in a single state, albeit shared with other South Slavic brethren (the kingdom of the Serbs, Croats and Slovenes, renamed the kingdom of Yugoslavia in 1929, which coincided with the abolition of constitutional government and the inauguration of royal dictatorship), the main objective of ethnology as a national science was also reached (cf. Naumović 1997: 319–51). Thus, it is a legitimate question to ask whether a change of perspective would have been appropriate within a new state that was emphatically multi-ethnic in character. This is particularly true given that each of the constituent groups had its own history of national sentiments and was quite protective of their unique identities, some also of their political autonomy, built and preserved while under foreign rule.[1] Could a different 'supranational science', oriented towards building bridges and defining common denominators towards other ethnicities within the first Yugoslavia have helped prevent the build-up of ethnic animosities and religious hostilities that led to the World War II atrocities? Could it have, instead, promoted the creation of secondary, alternative and truly pluralistic Yugoslav identity, rather than the enforced one designed to 'serve to enhance Serbian domination'? Serbian ethnology is taken as an example, but, certainly the same question could be directed to other, notably Croatian,

[1] For a concise but comprehensive, unbiased analysis of the political situation in Yugoslavia in the interwar period cf. B. Jelavich (1989, vol. II: 134–57, 192–204). This two-volume work on the 'History of the Balkans' had a very strong impact on my own meandering path of self-searching for a more meaningful and challenging ethnological/anthropological paradigm. The book that covers three centuries of turbulent Balkan past and inter-related political, economic, social and cultural histories, together and from the viewpoint of each of its lands, provided me with a more holistic, relativistic, integral and relational grasp of processes from a bird's eye perspective.

ethnology as defined by A. Radić and his successors. On the other hand, it is difficult to imagine a differently oriented 'folk science' while the government retained centralist rule and while everything that was attempted towards building a homogenized, non-ethnically based society (for example the division of the country into nine provinces, *banovinas*, which cut through national territories and traditional loyalties) was perceived as Serbian hegemony, the source of grievances of all other constituents (Jelavich 1989: 200).[2]

Therefore, the mission, trends, goals and techniques of the pre-World War I ethnology were carried on with little or no innovation. Diligent field researchers, professionals and literate individuals, mostly teachers, living in small towns or villages, continued to 'fill in the blanks' collecting data and producing monographs of the areas that had not been previously covered, using always the same questionnaires earlier compiled by the leading scholars,[3] never adapting them to the changed socio-economic environment or broadening the scope of inquiry. As a rule, their aim was to reconstruct the past, 'to save from oblivion' the former way of life, which implied 'the right way' of living one's life. The repetitive accumulation of data was not accompanied by theoretical or methodological growth, which would not only

[2] 'The boundaries of banovinas were gerrymandered so that Serbs formed a majority in six, Croats in two, Slovenes in one and Muslims in none. The principle offices in the state continued to be held by Serbs from Serbia, with the Slovenes receiving an adequate share. Muslims were allotted the lowest positions, whereas the Croats remained inadequately represented' (Jelavich 1989: 200–1). In effect, outside of Serbian majority, other nationalities, notably the Croats, objected to the national hegemony, political repressiveness of the regime and the existence of a unitary state. Instead, a federation of national entities with autonomous rights was preferred. Within the context of increased international dangers and with fascist and national socialist ideology echoed in the development of some internal movements within all national groups, national tensions were also growing. As an attempt of a belated remedy, the Croats, and only them, were granted autonomous status in 1939, but by then national conflicts have taken their toll. Pre-war animosities were carried into World War II and the concurrent Yugoslav revolution in which they have reached the extreme. Committed atrocities have created deep, painful and fearful memories, which could not have simply disappeared after the war. The communist regime has never successfully dealt with the problem of the past ethnic conflicts or attempted to promote a Yugoslav national identity. Instead it enforced the 'brotherhood and unity' concept and used the existing differences and historic memories for political manipulations and gains whenever appropriate. These same memories had been triggered off and used for the mobilization of masses during the postsocialist era 'wars for succession' that ended the existence of a country known for more than sixty years as Yugoslavia.

[3] The first questionnaire, 'Instructions for the research of settlements in Serbia Proper and other Serbian lands', was composed by J. Cvijić in 1896. Cvijić continued to publish instructions covering many other phenomena (different aspects of culture, migrations, ethnopsychological traits) as did his successors, first professors of ethnology at the University of Belgrade, Tihomir Djordjević and Jovan Erdeljanović, then some of their assistants and collaborators.

have provided a framework for interpreting the collected material (Kovačević 2006: 47–50), but also targeted research towards new topics and problems. Had new research paradigms been developed, different and additional data which would have described some of the actual events and processes (for example, randomly selected, detailed analysis of the effects of cash economy, depression, penetration of political parties into village life, the *function – not survivals* – of magical practices based on animistic concepts in contemporary culture, urban and government influences, the changing role of women and so on) could have been collected.

The discipline was perhaps dominated for too long (almost four decades, 1906–1944), by only two professors, Tihomir Djordjević and Jovan Erdeljanović.[4] Although those two professors may be considered as accomplished as far as their own research and publications are considered, within the chosen conceptual framework, already absolute in the inter-war period, during their time the Department of Ethnology did not grow; instead, it was more like a 'nursery' of talented students who worked as assistants for several years, before or after graduation, while waiting for a permanent position somewhere else to which they could be 'transplanted' (Rakić 1997: 304–5). Most of them created the pool of working ethnologists after World War II, some leaning more towards anthropo-geography, others towards ethnology, but all within the same established research tradition. Personally, I find the absence of development in the Department very intriguing. It seems to have been a method of controlling and preserving a single paradigm, as opposed to allowing pluralistic approaches to develop. Somehow, it is hard to imagine that no significant inspiration came from the challenges anthropology was dealing with in other countries, each in its own ways: in Great Britain (Radcliff-Brown in Oxford, Malinowski at the London School

[4] All of them, Cvijić, Erdeljanović and Djordjević, conducted impressive research, collected vast amounts of empirical evidence, published extensively and achieved some admirable results. They all studied abroad in the early twentieth century and were awarded doctoral degrees (Vienna 1893, Prague 1905, and Munich 1902, respectively). They continued to follow international literature, but new theoretical and methodological challenges as offered by contemporary British, French and American anthropology were ignored. It seems that they were selecting the works that confirmed their preconceived results. Thus, Erdeljanović (1938) learned English late in his career so that he could find writings that would support his thesis on animism as the earliest form of religion (evolutionistic approach) and prove his colleague Djordjević (1933) wrong in propagating the idea of primordial monotheism (a borrowing from German diffusionism and its *Kulturkreise* classifications). However, I have to note, that Djordjević deserves credit for introducing the use of archival material in the study of Serbian culture in the past, certainly a more viable source than peoples' memories of memories passed on to them from their elders. I am not denying the importance of memories, but as a source of penetrating into perceptions and value systems, and not necessarily for accurate reconstruction of past events.

of Economics); in USA (Franz Boas at Columbia University, Leslie White at University of Michigan, Redfield in Chicago, Kroeber and Lowie at Berkley); or France (Durkheim, Mauss, van Gennep, Griaule). Was it because they were mostly (but not exclusively) oriented to the study of 'other' cultures, or because the names of the discipline, anthropology and, in the French case, sociology, were considered distant and unrelated? Perhaps it was because younger ethnologists did not spend extended periods of time at universities abroad learning about new developments at their source. Perhaps it was because the approach of Western scholars was too radically different to be perceived as applicable to the native scientist cause.

It is not only that those modern influences did not inspire Serbian ethnology through literature, but even direct live contact between representatives of domestic and foreign disciplines did not spur any interest to try out a different approach. I am referring to the presence of American scholars who had began to exhibit an interest in Balkan cultures and field research *in situ* as early as the 1930s. The works of three scholars belonging to three different, but related disciplines have left a lasting legacy and an important contribution to the body of knowledge about Balkan societies and cultures presented in the English language. Those were: sociologist Irwin Sanders, folklorist Albert Lord, and historian and political scientist Phillip Mosley. Sanders began his work in 1935 in a Bulgarian village Dragaljevci, near Sofia, focusing on the study of interactions among social groups. His monograph, *A Balkan Village* (1949), based on the novel community study approach, strongly influenced future research by American scholars working in the Balkans. Albert Lord visited various parts of the Balkans, from Bosnia and Herzegovina to Bulgaria over a twenty-five year period (1934–59) and recorded a vast number of oral epic and lyrical poetry, now part of the Harvard University collection. In addition to analysing and interpreting poetry, Lord focused his research on studying the performer, 'the singer of tales', determining how he remembered, reconstructed and transmitted a poem within its living socio-cultural context. Direct contact with the culture, the search for universal patterns ('formulae theory'), comparative perspective and theoretical generalizations made Lord 'an anthropologist of oral literature'. Finally, Phillip Mosley[5] travelled the Balkans and by using the uniform case-study method collected and analysed data on various types of

[5] As Phillip Mosley was getting ready to start studying diplomatic relations concerning Dardanelles, it was recommended to him that instead he should go to the Balkan Peninsula and study the living cultures there. Since he was coming from a different field, he consulted with Ruth Benedict and Margaret Mead at Columbia University, and most importantly, went to the London School of Economics to study fieldwork methods with Bronislav Malinowski. With this, he became highly qualified for the research he was about to undertake.

zadruga, the flexible, alternative extended family organization. Instead of attempting the romantic and patriotic pseudo-historic search for the origin of the institution, or its ethnic and religious specifics, like native ethnologists, his goal was to determine internal structure and functions of a *zadruga* as a social instrument of organizing labour, providing security and adaptability to the environment in a combination of ideographic and nomothetic approaches (Mosley 1976a–c).

All of these novel ways of looking at and understanding a peasant society and culture landed on deaf ears. The only exception was Milenko S. Filipović, who had met Mosley while in Yugoslavia, and who understood the need to learn about these new, alternative approaches. In 1939 he applied for a scholarship from the Rockefeller foundation to go and study with Malinowski at the London School of Economics. The outbreak of World War II prevented him from going. Nevertheless, his desire to study abroad was granted after the war, in the early 1950s, but instead of going to England he travelled to the United States.

The End of an Era and Continued Stalemate

During World War II the discipline suffered considerable fatalities. The Department lost both its senior members of staff and there was no one of the same calibre and experience to take their place. In addition, the entire seminar library was destroyed in a fire deliberately set by the German occupying forces. Until Borivoje Drobnjaković, director of Ethnographic Museum in Belgrade, was hired as a professor in 1950, the entire burden of re-opening the department, replenishing its library, and teaching many different classes fell on the shoulders of Mirko Barjaktarović who was an assistant to Erdeljanović for just one academic year before the war, and who was rehired in 1947 (Pavković 1997: 1–5).

However, one of the gravest threats came from the drastically changed socio-political context within which ethnology was to operate after the communist regime came to power in 1945. There was no reason whatsoever for ethnology to retain its privileged status as a 'national science' any longer. The new authorities that placed themselves in charge of the definition and management/manipulation of the 'ethnic issues' (see Prelić, this volume) had no use for traditional ethnological research topics, such as the continued delving into the nation's past and pursuing the research of its ethnic origins and history. This was not favourably looked upon especially if it came from the Serbian side, since the Serbs, who represented the overwhelming majority in Yugoslavia, were still associated with the pre-war hegemony and the

pro-royal and pro-Serbian Chetnik resistance movement during the war.[6] The regime felt that it had to keep potential Serbian nationalism and everything that could revive or promote it under firm control. To make things even more miserable for ethnologists, the regime was not a big fan of peasantry either, the main subjects of ethnological research. It strongly preferred progressive industrial workers (proletariat) to backward small land holders living their own, self-sufficient lives in scattered and not always easily accessible villages.

Ethnology was not theoretically and methodologically equipped to study what was considered desirable and progressive 'from above'. Instead, sociology[7] and, strangely enough, folklore research[8] were the ones that met

[6] Chetnik detachments were formed by Serbian officers after the surrender of the Yugoslav army in 1941. They were loyal to the King and the government in exile and they fought for the 'Serbian cause' on both national and especially anti-Croat and anti-communist platform. The rivalling guerrilla groups, the Partisans, were the major multi-national resistance movement. Under the leadership of Josip Broz Tito and the Yugoslav Communist Party, they were in the end victorious, which also meant the defeat of the Serbian nationalistic programs (Jelavich, vol. II, 1989: 267–72).

[7] Sociology was first established at the University of Belgrade Law School in 1935. After World War II it was stigmatized for a time as bourgeois science and it took more than ten years to formally reinstate the discipline. First it became a part of the Institute for Social Sciences founded in 1957, and two years later it was added to the Department of Philosophy at the University of Belgrade. Very soon afterwards, it became a prominent, well-financed discipline not only because it was able to provide meaningful insights into specific social issues, but also because it was rapidly developing into a solid, theoretically and methodologically grounded science with well-defined concepts and goals. Its social and scientific relevance attracted far greater numbers of students than ethnology. As sociology was developing as a discipline, it was also 'infringing' areas of traditional ethnological research: rural sociology, demography, social anthropology. The empirical and theoretical levels attained were much higher than in ethnology. Sociologists spoke other languages, attended international conferences, encouraged free flow of academic ideas and engaged in professional networking. Therefore, it did not come as a surprise that foreign anthropologists more readily established contacts with the Department of Sociology, often completely bypassing ethnologists with whom they shared very little academic interests. It was also because ethnologists carried very little social and political weight and they could not make 'things happen' as efficiently as others could. It should be noted, however, that the roles of sociologists and philosophers were complex and ambivalent, to say the least. In the 1950s and 1960s they were mostly ideologically committed to the needs of the regime. Later on some of them continued to perform services for the government but some of their most prominent disciples were also its biggest critics, because of which they were treated as dissidents.

[8] A group of folklorists in the Serbian Academy of Sciences and Art, under the leadership of Professor Dušan Nedeljković, a philosopher, was engaged in collecting new folklore creations, such as songs sung by Partizan during the war and workers while building a new socialist state after the war. Of course, the ideological loyalty and actual role of some scientists with political clout was rarely revealed to the public. There were, however, always hushed rumours, and Nedeljković was never portrayed in a positive light.

the challenge. Ethnologists on the other hand, faced with fast-paced change, strongly believed (or did not know better) that their duty was above all to record whatever was still left of the 'pure', 'authentic', 'uncorrupted' and 'uncontaminated' folk culture before it was snatched away by 'progress'. Of course, in order to fulfil that goal they had increasingly to rely on their informants' memories (always searching for the oldest villagers) rather than on what they could in fact see and record as an actual socio-cultural reality. In this way, and by opting to represent the idealized, conflict-free image of peasant society, in a static, timeless way, they, like their predecessors in the inter-war period, regretfully, missed the opportunity to record the invaluable evidence of how the villages were handling the change inflicted upon them and what kind of processes were evolving with the onset of rapid modernization, industrialization and massive exodus of the village population into urban areas.[9]

Not only were the ethnologists[10] going against the tide, but they were also performing quite poorly, losing scholarly credibility with their choice of topics ('the study of men's drawers' was one of the disparaging descriptions of ethnographic research), data-collecting process and presentation as crude, non-interpreted evidence. This is a very general statement, but that is how ethnology was perceived and criticized from the outside, by representatives of other social sciences. Belief that the best possible ethnologist could be somebody who was socialized within village culture and that the place of his/her geographic and social origin qualified him/her for the profession better than any training could do (confusing ethno-explication with scientific interpretation – Kovačević 2006: 55),[11] did not help the reputation of ethnol-

[9] Ethnographic Institute of the Serbian Academy of Arts and Sciences, founded in 1947 after the Soviet model, had in its programmes the study of cultural dynamics and processes and effects of modernization. The largest project the Institute did in this venue was the study of the Djerdap area in Eastern Serbia where a huge power dam on the river Danube was to be built. However, the project was done very differently than similar endeavours in other parts of the world. It was more, as in the case of Jarmenovci (Knežević and Jovanović 1958), a village in central Serbia, a reconstruction of what might have been, than of what it was at the time of research. In Jarmenovci, for example, instead of studying the effects of electrification and the impact of a United Nations developmental programme that was implemented there, the authors produced a reconstruction of timeless village culture with the results that were not different than if the same village had been studied fifty years earlier.

[10] As always, with some notable exceptions: for example, the work of M. S. Filipović; his Ph.D. student Manojlo Gluščević and his study of couvades; later Djurdjica Petrović who developed innovative approaches not only through foreign influences but also through cooperation with history and history of art; Nikola Pavković established bridges towards history and traditional law studies, and so on.

[11] I must emphasize that in the years after World War II, the favoured 'peasant origin' of Serbian ethnologists was more an ideal than a reality. Before the 1970s within the university department, ethnologists of peasant or, more precisely, small-town origin prevailed, while

ogy among colleagues from other disciplines or in society at large. Hence, they were marginalized, deprived of any social influence and almost never consulted in problem-solving or conflict-resolution situations that were within their expertise. Growing insecurity about most of the aspects of their discipline, led to ethnologists' gradual (self)-isolation from other scholarly disciplines. One of the indications that their science was 'in a stagnant or critical state' was that 'in its entire history there have never been more texts about the definition of ethnology, its subject and relations with other social sciences than in the period between 1955 and 1975, when [ethnology] was clinically dead' (Kovačević 2006: 38–9). One positive effect, however, was that the authorities never really bothered them and allowed them to 'do their own thing', to engage in harmless pursuit of antiquity.[12] Or, the authorities

after that period they represented a definite minority, almost an exception. However, perhaps as a way to reinforce the endangered ideal, several professors as late as the early to mid 1970s still professed the importance of being of peasant origin not only for cognitive but also practical reasons. Rural ethnologists could perhaps prevent the urban ones from going astray with their research topics and therefore threatening the 'autonomy of the well-defined discipline'. Dj. Petrović was used as an example of the latter; a disparaging phrase, 'guns and pianos', was coined to discredit her work. She studied Balkan weaponry through a historical perspective and discussed in one article the appearance of the first piano in Serbia. Both of these were considered 'non-ethnological' topics.

[12] Of course, in an authoritarian state (or maybe, in any kind of state) there are always at least some subtle forms of control in important parts of social life. Higher education was certainly one such area. Members of the Communist party were, I am convinced, although I do not have reliable insider information, responsible for making sure there were no dissident activities in progress. That is why every department was supposed to have one or two faculty members (approximately 5 to 10 per cent in the case of the small Department of Ethnology) who were party members. If they were to witness major infringements, I believe they would have probably had to denounce their dissident colleagues. I do not know how it was in other departments, but our 'guardian angels' have never publicly exercised any of their 'rights' or 'obligations' (maybe because the rest of us were so hopelessly apolitical, or at least we thought we were), but I always believed that if they had to take such action it would have been to protect us from the regime, rather than the government from us. This is especially true where there were no personal animosities, rivalry or grudges. However, as in the Kulišić – Filipović case, personal relations burdened with vengeful sentiments could lead to political manipulation. Our colleagues at the Department who also happened to be party members were normal people with strong collegial sentiments and no harm could have come from them. Nevertheless, only they can answer if either of them was in any trouble with the Party because of any of us. The majority of faculty and staff never thought of them as party members and were never 'careful' in their presence. After the purges in the mid 1970s when eight professors and assistants were expelled from the Department of Sociology and Philosophy for participating in 1968 student demonstrations, we all wondered if there were, especially since we had just then moved into a new building, any recording devices installed. Once I was given the following explanation: 'Offices may be bugged because one can have secret conversations there. Classrooms are bug-free because there is no need for them: somebody from the student body (or an implanted non-student) will file a report if, in their judgement, anything went astray.' Personally, I never had a problem during socialism, but I did have an unfortu-

could leave the ethnologists alone, because they helped their demise in the early post-war years.

It became obvious that ethnology, deeply entrenched in its fossilized paradigms, was unable to break the vicious circle in which it found itself. Insights or borrowing from other related disciplines that could have helped the change of perspective, were lacking (not on an individual, but on a collective level). The other possibility was to open doors wide to the influences coming from ethnology and anthropology as they were conceived and practised in some other parts of the world. In the mid 1950s Serbian ethnology was potentially exposed to two such options, represented by two native scholars, Špiro Kulišić (1908–89) and Milenko S. Filipović (1902–69), who were at the opposite ends of the spectrum professionally, epistemologically, ideologically and politically. Kulišić was the representative of a hybrid theory based on evolutionism and historic materialism (Marxist theory) which was the official theory widely used in the Soviet Union and other communist countries. While providing service to the government in attempting to enforce its socialist programme and creed, he applied his dogmatic and ideologically charged version of the theory to the construction/reconstruction of archaic study of familial, kinship, and tribal organization and ethnic relations in the Dinaric region, and the alleged agrarian and matriarchal roots of Serbian folk religion (cf. Gorunović 1997). As could be expected, he was intolerant towards other approaches in anthropology, especially towards British functionalism and structural functionalism, denouncing them, following the Soviet lead, as anti-historical and imperialistic–colonial. Although he accused other scholars of advocating pseudo-scientific, reactionary views, his main attack was focused on Filipović, whom he regarded as a strong competitor and a threat. Unfortunately, the entire conflict was not so much about scholarly polemics or even the defence of 'progressive thought' against 'bourgeois survivals' as it was about careerism and personal resentment (see Gorunović, this volume). Although Kulišić's ideological attempts have failed since his theory was neither mandatory nor did it secure a large spontaneous following, pointing a finger at a colleague with pro-western orientations in the 1950s, was a serious accusation which could have had grave political implications.

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nate incident during the nationalist euphoria in early 1990s. I do not know who the denouncer was, nor did I ever attempt to find out, but he or she was obviously a novice to the business, or was simply trying to win some kind of points for him/herself. The 'report' had no bearing whatsoever to what was said or discussed in class. Still, it was a very unpleasant experience revealing to what extent things had changed: the trust between student–teacher and colleague-to-colleague communication was suddenly and sadly lost.

> My discontent with the situation in ethnology and my attempts to change it so that we, Serbian ethnologists, could start to study the real life of our people, instead of antiquities, date quite a while back. However, the main result so far has been the fact that I had to leave Belgrade and retreat to Sarajevo[13] where I have tried to do the same thing, again in vain. Because of my stand that ethnological research should be carried out in conjunction with the real life problems, I have been stigmatized as a reactionary and an exponent of Anglo-American imperialism! However, there is hope. There are some younger colleagues who want to do better. Institute of Hygiene and Bureau of Statistics are starting some interesting research. Recently the Institute of Social Sciences was formed which has in its programme the study of family, social change, especially in rural areas and many other topics. Prominent Marxist theoreticians [different from Kulišić's dogmatic Marxism] from the Institute have asked me to collaborate with them. I have accepted, in spite of the opposition from the so-called 'Belgrade [ethnological] School' (Filipović's letter to Joel M. Halpern, 17 May 1959).[14]

Thus, it seems that Filipović's greatest 'sin' was his appreciation of American (and Western) anthropology with which he became familiar through personal contacts with American scholars.[15] Although he was Cvijić's stu-

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[13] Milenko Filipović began his career as an assistant at the University of Belgrade (1924–6). He was then an associate professor (docent) at the University of Skopje (1930–41), University of Belgrade (1941–3) and full professor in Sarajevo (1955–62). From 1945–50 he was a curator at the Ethnographic Museum in Belgrade, and Research Fellow at the Ethnographic Institute at the Serbian Academy of Sciences and Arts (SASA).

[14] J. M. Halpern has retained his entire correspondence with Professor Fillipović from 1954 until his death in 1969. The collection that consists of 151 items is now digitalized as a part of Halpern's archive located in several institutions around the world. This is a very valuable source for the study of Serbian–American scholarly relations that covers a period of fifteen years, even though it is limited to a single 'case study'. Only a small fraction was used in this chapter.

[15] Filipović and Halpern had devised a very efficient way of exchanging news in the disciplines on opposite sides of the Atlantic, as well as publications. There are many letters in their correspondence dealing precisely with this matter. Filipović sent Halpern publications he needed while writing his dissertation, and, in return, Halpern made sure that all Filipović's subscriptions were current so that he could continue to receive American journals: *American Anthropologist*, *SW Journal of Anthropology*, *Journal of American Folklore,* and *Rural Sociology*. Specifically, in a letter dated 6 August 1954, Filipović requested the following books: L. White, *The Science of Culture*, J. Herskowitz, *Man and his Works* and L. Beals and H. Hoijer, *An Introduction to Anthropology*. I was quite impressed that in the mid 1950s there could be such an unobstructed exchange of books through regular mail and that Professor Filipović was able to receive all his magazines and books, perhaps after some careful inspection at the customs.

dent and follower, and never rejected the native paradigm, he was astutely aware that alternative and, in his view, complementary approaches to the study of culture modelled on Western (American) anthropology could have positive effects on the development of ethnology at home. He was in favour of shifting research from the past to the present, from a static to a dynamic perspective, from distribution and (pseudo-)evolution to function and structure, from reconstructions to the search for causes and consequences. He went on to propose that the results of ethnological research should have practical applications in solving social issues. Filipović criticized even the most sacred aspect of all, the way of 'doing ethnography', recommending that, instead of random accumulation of data, information should be collected with the research objective. In addition, he pleaded, field research should concentrate on the present, for what was seen and registered as 'now' will be the 'ancient past for the next generation, but it will be readily accessible to them because of our work' (Filipović 1955: 214).

What he urged in his programmatic articles, Filipović applied to his own research. He published extensively leaving a lasting source of influence, at home and abroad.[16] He opened up or revolutionized the approach to the study of many important issues, such as ethnic groups, the origin of tribal society in the Balkans (the so-called Vlach theory), social rituals such as '*Zvanica*, a Serbian Institution of Reciprocity in Rural Life' (1965), 'Suicide in the Takovo Region' (1958), (so much for the idealized, conflict- and tension-free representation of village life in traditional ethnography) and many others. Even his monograph *Takovo* (1954), about a region in central Serbia (Šumadija), was different. All of his work after his initial contacts with Professors Albert Lord and Phillip Mosley in pre-war Yugoslavia, and especially after his yearlong sabbatical in the USA, at Harvard (Lord, possibly also Talcott Parsons, a follower of Durkheim and Weber also worked there at that time), with frequent visits to the Columbia University (Mosley, Arenberg and their student Halpern who Filipović cherished as a colleague and a personal friend until his death), novel in theme, concept and methodology, began to resemble the work of American anthropologists.[17] Unfortunately, very few Serbian ethnologists at that time followed in his footsteps.

[16] Filipović's bibliography for the period 1924–60 consists of 280 monographs and articles published in Yugoslav and foreign journals, including a number of British and American publications (e.g. *Man* and *Harvard Slavic Review*). Since his death, Professors Hammel, Halpern, Lord, Echrich and Filipović's daughter, Radmila Fabijanić, have edited, translated and published a collection of his papers, *Among the People* (1982).

[17] During his stay in the USA Filipović also gathered material on immigrant Serbian population in America and planned to publish an extensive volume on the subject. Perhaps the manuscript still exists in his private collection (personal communication with Professor Halpern).

'Retreat' to Sarajevo curtailed his opportunity to emanate his influence at the largest centre of Serbian learning, Belgrade. However, although he may not have been able to act officially, through an institution,[18] he was very well respected among younger colleagues who desired change, and his home was always open to anyone seeking his advice.[19] The value of his work has passed the test of time and it is still admired even by today's youngest generation of native scholars, well versed in anthropological theory and practice, past and present, in all parts of the world. If Professor Filipović could see the changes made in Serbian ethnology/anthropology since the mid 1970s he would have been proud and content that his pioneering steps and sacrifice were not in vain. Actually, there is still so much more we can all learn from his work.

The Halperns in Orašac

What was it in American cultural anthropology that had attracted Filipović and made him believe that it was a path to consider? Since the war, the anthropologists who have spent the longest time in Serbia studying its culture by focusing primarily on one village, Orašac in Šumadija, were Joel and Barbara Halpern (see *Interview*, this volume). They had, in a way, combined the efforts of all three of their predecessors (Sanders, Lord and Mosley) and kept adding complexity and innovation as time went by: community study, kinship and social structure, social change, personality and culture, historic demography, life histories, expressive behaviour and oral tradition from a socio-linguistic perspective.[20]

Through his undergraduate and graduate studies Joel M. Halpern was exposed to many different intellectual influences and anthropological

[18] He did mentor one of the best doctoral dissertations, on couvades, prior to 1970s by Manojlo Gluščevič, at the University of Sarajevo.

[19] I remember my mentor, Professor Djurdjica Petrović, certainly one of the most prominent *enfants terribles* in Serbian post-World War II ethnology, who not only contemplated change but took decisive action to alter her path, telling me how Filipović always ready to listen, advise, critique and offer support. Filipović, contrary to his arch-opponent, Kulišić, was open to many different approaches. He did not believe that his way was the only possible way: on the contrary, as long as a researcher was breaking out of the vicious circle and aiming for higher standards. Djurdjica Petrović's path was into the study of aspects of everyday life in different historical epochs via archival documents.

[20] There were many other anthropologists who had conducted field research in Serbia from the 1960s until the present (E. Hammel, A. Simic, B. Denich, R. Wagner, J. Foley, R. Hayden, M. Bakic-Hayden, E. Gordy and others). Limited space, regrettably, does not allow their superb scholarship to be mentioned here. After all, the aim here was not to offer an overview of American research but to determine what was in the theoretical and methodological package that American anthropologists brought with them in the 1950s when ethnology was in its deepest crisis and in dire need of inspiration.

schools of thought. He pursued his undergraduate studies in history and anthropology at the University of Michigan' There, the discipline was dominated since 1930 by Leslie White, who had developed a materialist, neo-evolutionist theory in direct opposition to Boas. Halpern then went to the Columbia University to study at its Russian Institute. The Department of Anthropology at Columbia was the birthplace of the American unique four-field discipline,[21] developed by Franz Boas, a German born and educated natural scientist turned anthropologist. He dominated almost the entire American anthropology for a long time, from his first year at Columbia in 1899 until his death in 1942. Inspired by Herder's Romanticism and the teaching of Adolf Bastian, Boas actually transplanted the German intellectual tradition to the USA, giving priority to diffusionism over simplistic and schematic evolutionism and to systematic and detailed research of particular cultures over empirically insufficiently grounded generalizations. He therefore advocated cultural particularism and relativism, but also, like his colleagues in France and Great Britain at the same time, a belief in a holistic approach according to which cultural elements could not be understood in isolation but only within their context (cf. Eriksen and Nielsen 2001: 38–41).

By the time Halpern came to Columbia, many changes had already taken place. Boas's students, Ruth Benedict and Margaret Mead had developed the 'culture and personality approach' and the studying of 'culture at a distance', by using various sources and interviewing immigrants from inaccessible lands (e.g. Japan during and after World War II,[22] or the Soviet Union after the Revolution). In 1946 Julian Steward came to develop his materialist, historically oriented neo-evolutionism and to end the anti-theoretical mode advocated by Boas. He was replaced by Halpern's future mentor, Conrad Arensberg, in the early 1950s who, together with Karl Polanyi, Columbia's economic historian, developed methods of analysing the economics of ancient empires and other issues.

The Russian Institute, jointly founded in 1946 by Columbia University and the Rockefeller Foundation and headed by Philip Mosley, also contributed to a change of emphasis in anthropological studies and their movement further away from Boas's model at his own school. It first of all introduced hitherto almost totally absent interest in politics and economics:

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[21] Those are: archeology, linguistics, physical and cultural anthropology. Anthropology students in the USA start their training by studying all four fields, and then they specialize in one of them.

[22] 'The Chrysanthemum and the Sword' on Japanese culture was created exactly in that way. Had it not been for Filipović and the possibility of having first-hand field experience, Halpern would have continued to study, as part of a large project based in Harvard, Russian culture at a distance by interviewing displaced Soviet citizens in USA or Germany.

> …it was the Second World War and the post-war responsibilities of the United States as the strongest power of the free world which made clear the need for the systematic and many-sided study of major world areas outside the United States and Western Europe. One result of this awareness has been the development of 'area studies', programmes of study and research directed to enlarging our knowledge, not only in Russia, but also of the Muslim world, the Indian sub-continent, Africa, China, Japan, and Latin America … The Russian Institute conducts an intense programme of training for graduate students (Mosley 1954: 1).

As the 'areas of study' were defined, it became obvious that there would also be a shift from investigating tribal entities to more complex peasant societies. The 'Community study approach' came to prominence as a methodological framework for analysing and describing rural and urban settlements, as segments of modern state, in a holistic,[23] functionally interrelated and integrated way, 'from a particular point of view, rather than a series of seemingly unrelated fragments' (Halpern 1956: 1). 'The community study attempts to do for the rural or urban unit of the modern state what the ethnography did for a tribe' (Halpern 1956: 3). This approach began to be developed and applied already in the late 1920s, first in the study of homeland small towns, Middletown (Lynd 1929); and Yankee City (Warner and Strole 1945; Warner and Low 1947), and then prior to, during and after World War II, in the study of rural settlements abroad. These newer studies were definitely more ambitious as it was realized that they could be 'very effective in investigating various social phenomena and theoretical issues' (Milić 1957: 48). Those were the well-known monographs by Robert Redfield (1941), O. Lewis (1951), J. Steward (1950), and Conrad Arensberg. Professor Arensberg's study, *The Irish Countrymen* (1939), was a classic ethnography of life in a European rural community. As Halpern's professor and thesis adviser, he had a direct and most profound impact on Halpern's

[23] 'It would seem that when anthropologists go out into the field to study culture, especially one that has been little described in literature, they have something of an obligation not only to secure data bearing to their problems or in congruence with their approaches, but to attempt to describe the culture as a whole, so that for the future we will not have only bits and fragments but at least the bare outlines of the culture unit, since it is manifestly impossible to describe any culture *in toto*… Since the observer is painting a picture of a culture at a certain point in time, it might be well to be conscious of the problem of including sufficient descriptive data, so that investigators in the future may be able to use the same material with perhaps different theoretical purpose in mind' (Halpern 1956: 2, 8).

research in *A Serbian Village*[24] that has been published in three different versions (1956, 1958, 1967), plus the latest one in Serbian translation (2006).

As the subject of the American anthropologists' research ceased to be Native American and Inuit populations living on their own turf ('anthropology at home'), patterns of fieldwork had to change as well. Opportunities to study overseas culture *in situ*, required the kind of fieldwork inaugurated by Malinowski – long-term, stationary investigation, employing participant observation, far away from home – that was adopted and adapted by a number of American anthropologists in the 1930s. At Columbia University this type of fieldwork was promoted by Margaret Mead who gained first hand experience while studying the cultures of Samoa and New Quinine.

The goals of collecting and interpreting information from various cultures also changed. While the requirements for the 'community study approach' to comply with the holistic principle was in accord with Boas's teaching, concerns about cross-cultural comparability, which implied at least a limited amount of standardization of data-gathering procedures was very advanced in relation to his beliefs. Reflective of the need of anthropology to aim 'not only to understand cultural wholes (a Romantic enterprise), but also to dissect, analyse and compare them (an Enlightenment project)' (Eriksen and Nielsen 2001: 12–13), was George P. Murdock's project to create a worldwide database of cultural information that could be used for comparative generalizations. The database was named after the school in which it was housed, the Yale Documentation, or, descriptively, Human Relations Area Files (1937). In the original database information on specific topics were coded and classified on paper and, in the next phase, transferred to microfiche.[25] Although there have been concerns, controversies and criticism about this programme, it continued to grow and diversify into several different, computerized subsystems.

Another important development in American cultural anthropology in the years before, during and after World War II, was the possibility of applying research results in solving social problems. C. Arensberg was one of the pioneers in this area, but the same aspirations can be found in the work of other anthropologists of the time, notably R. Benedict and M. Mead. While some of the work was directly related to the needs of the government, such

[24] Halpern asked Professor Filipović to read critically the manuscript of his thesis successively as individual chapters were written. Filipović readily agreed and edited the entire manuscript very meticulously. One of his strong recommendations was to change the original title 'Life in a Šumadija Village' to 'Life in a Serbian Village' for the sake of American readers who 'will probably have trouble locating Serbia, let alone Šumadija' (Letter to Halpern, 10 May 1955).

[25] Some of M. Filipović's research results were added to the Serbs files.

as the need to understand the Japanese 'national character', or boost national morale during the war (for example, R. Benedict was on the Committee for National Morale), other was applied in promoting social action such as in education, ecology or the women's movement. This specific venue of cultural anthropology could have been the source of concern outside the USA and fear that data gathered during field research in a foreign country could be used for additional, other than purely scientific needs. What is defined as patriotic within may be read as 'espionage' without.

In their study of Serbian village life, the Halperns employed the majority of the theoretical postulates from mainstream American anthropology, which, in my view, resulted in an excellent monograph greatly different from ethnographies written by native scholars.[26] Limited space does not allow me

[26] Recently, however, an opposite view has been expressed after comparing Halpern's *A Serbian Village* and the *Jarmenovci* monograph by Knežević and Jovanović (1958). In this article the overall value of both of these books as an ethnographic source is questioned. Some of Kovačević's stands require a serious dialogue, which, regrettably, cannot take place here. For instance, he postulates that native ethnologists, because of their background knowledge could gather all pertinent information in a short period of time, in ten days to two weeks, and produce an ethnography that would be equally 'informative' as the one generated by an anthropologist-outsider after a year of participant-observation. He further argues that the differences between the two disciplines do not really account for much because the final product of both of them, the monograph, suffers from common maladies, for example: it does not offer precise rules of how data were collected, that is well-defined methodological and epistemological standpoints; it is not precise in recording the timeframe but blurs the history and process with reporting in 'ethnographic present'; does not distinguish between 'norm' and 'ideal', and so on. In essence, he is very much against writing ethnographies (especially post-modern 'writings of culture') and instead he is in favour of deliberately created 'fragmented data that would be an equivalent to historic source'. In other words, collected datum, accompanied with detailed description of how, from whom and for what reason it was gathered, would be catalogued and printed/stored with other fragmented pieces of information in a database. Anthropology/ethnology should do away with ethnography, a written report and construction of fieldwork experience (cf. Kovačević 2006: 97–145). Obviously concerns about contextualized meanings, dynamics of interactions, or advantages of a holistic approach are not considered important. The proposed 'data collection' actually describes how the data, extracted from written monographs, is organized and presented in HRAF databases. The novelty is that the field researcher would only have to submit his field notes to a data processing centre without having to go to the trouble of writing a book about his findings. I will let the readers evaluate the merit of this proposal. One of my associates, while reading about this proposal remembered how a lecturer at the Department of Ethnology in the late 1960s and early 1970s, who, ironically, happens to be one of the authors of *Jarmenovci*, gave us similar assignments. She would ask us (freshmen and sophomores) to study one or more of the famous anthropological monographs, but instead of expecting a discussion about their authors' methods and results, we were required to 'deconstruct' the text in such a way so that we could fill in the pre-set rubrics, such as dwelling types, footwear, basket weaving, wedding rituals etc. with the corresponding data extracted from the book, and 'learn' about cultures in such atomized, disconnected way. I remember reading Margaret Mead's *New Lives for Old*, her restudy of the Island of Manus. I was at the same time fascinated with the concept of

to enumerate these differences. Suffice it to say that the aim of the research was not simply to describe village life at a particular time in history in an area that had been virtually unstudied by the methods of American anthropology, but also to channel analysis towards specific theoretical goals which were again a corollary of the American version of the discipline in the early to mid 1950s. One of these goals was to 'attempt to depict a national character in terms of the community study approach … and to determine the 'typicality' [of that village], relative to the rest of Serbia and even Yugoslavia' by means of comparison (Halpern 1956: 1, 12). Another important objective was to study, again through the community study approach, cultural and social changes impinged 'upon the village both from within and without'. The distinction refers to the changes brought about as a direct consequence of particular government policies, or processes of modernization (industrialization, urbanization), as well as to those that 'have occurred indirectly, as part of an overall process of unplanned cultural transformations in Serbia in general during the past century' (Halpern 1956: 17). *A Serbian Village* created an interactive image of village life at a certain time period. In time it became a memory of a moment long gone. It was an excellent baseline cross-section of a national rural culture from which the authors could branch out in time, both into the past and into the future through subsequent research that lasted almost fifty years (longitudinal studies). They also branched out methodologically, theoretically and thematically, specializing and following their own interests as well as the ever-changing field of anthropology: historic demography, cross-linking of linear and cyclical time, oral tradition from sociolinguistic perspective, medical anthropology and so on.

Parallel Paths Do Not Cross

Halpern readily acknowledged that Serbian ethnology and anthropogeography have influenced his work to an extent. This is especially evident in the attention he pays to material culture (clothing, shelter, tools, diet), regional differences and cultural history, usually absent from classic American monographs. It seems, therefore, logical to assume that the emphatically different approach and goals of American scholars researching the same phenomena as domestic 'national scientists' and on their own turf, would have aroused the keen interest of these scholars. Regretfully, however, both before and after World War II, there was mostly polite detachment and disregard for what foreigners were doing. Native scholars, as cordial hosts,

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measuring change that had occurred between Mead's visits to the island, which my professor did not seem to share, and frustrated because there were not many rubrics that I could fill in.

were ready to offer information on available sources and literature, help with administrative procedures, and establishing contacts with local authorities, but lucid scientific debates, exchange of ideas, students and scholars, or collaboration on joint projects and publications were all lacking. As noted earlier, American scholars found that they had much more in common with sociologists, political scientists, demographers, folklorists and authors than with ethnologists. There were many reasons for that, one of the most important being the fact that before the mid 1970s very few native ethnologists spoke English well enough to be able to follow English-language literature, and therefore have insights into the discipline. The language barrier had another negative effect: even if native ethnologist spoke some English, they lacked the proficiency needed to conduct nuanced scientific debate. The same was true for visiting anthropologists: their Serbian was sufficient for everyday communications and field research, but often deficient for discussions on abstract theoretical concepts.[27] For the same or other reasons (in my opinion, Serbs prefer oral to written communication in interpersonal relations), 'Yugoslav colleagues had almost never corresponded' (Halpern 1978: 5). Filipović was a notable exception to this rule.[28] On the other hand, it is also possible that language was not the most serious obstacle in becoming familiar with foreign anthropological work. Rather, it may have been the lack of interest and initiative.

> It should be stressed that the interest and arrangements for translating most if not all American and West European anthropologists into Serbo-Croatian have not come from the more traditional ethnologists but from those interested in literature (Halpern 1978: 6).[29]

But the situation changed in time:

> It should be noted that the instructors [at the Department of Ethnology graduate students who are also fully employed as teaching assistants], unlike the older generation were fluent in English and knowledgeable of the English language literature (*ibid.*: 9).

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[27] It should be noted that the Halperns were well prepared and they delivered their lectures at SASA and other gatherings in the native language quite successfully.

[28] It is interesting to note that Halpern wrote letters to Filipović in English and Filipović always responded in Serbian 'so that you maintain the knowledge of the language (Letter to Halpern, 8 June 1954).

[29] It is absolutely true that there was very little ethnologists' involvement prior to the mid 1970s with the translation and publishing of capital anthropological works originally written in foreign languages. For the subsequent change, credit goes to the major publishing houses ('Nolit', 'Prosveta', 'Beogradski izdavačko-grafički zavod') and their editors. The longest lasting and most deserving of praise is the edition 'XX Century', conceived, published and now owned by linguistic anthropologist Ivan Čolović.

Another factor that might have played a role in impeding the cooperation that could have helped native ethnology redefine itself sooner was the fear or anxiety of possible negative consequences of contacts with foreign scholars. Fieldwork became especially difficult in the 1970s because of the political situation in the country and events that were an overture to the subsequent break-up of Yugoslavia. The 1974 Constitution, which provided legal grounds for the constituent republics to organize as separate states, made the internal borders far less permeable than before. Like many other domains, scholarly research was confined to the areas within a republic and this applied to both domestic and foreign scholars. For example, domestic scholars could only conduct research in a republic other than their own if there was an officially approved joint project in place. Similarly, foreigners needed to have separate fieldwork permits for each republic they intended to work in, to be associated with an appropriate scientific institution and accompanied by a domestic colleague while in the field. The situation grew far more serious towards the end of the decade:

> It is our firm impression that at no time in our 25 years of experience visiting and working in Yugoslavia have the conditions for scholarly research been more difficult ['worse than in 1948', as one official has put it]. Without question, academic and scientific institutions reflect what a Yugoslav colleague has called the 'security hysteria'. The most obvious reason for this is the impending demise of their charismatic leader Marshall Tito, who is now 86. Like all authoritarian and single-party states, Yugoslavia has no easy mechanism for passing on political power. Combined with this is a very real fear that the Russians will profit from the coming crisis and the ever-present nationality antagonism. To offset this, a policy of vigilance against all foreigners has been adopted, combined with increased emphasis on internal security (Halpern 1978: 41).[30]

As domestic colleagues were required to file reports after talking to foreigners, and even asked to provide additional information by direct contact with a security officer, some have refused to talk to American and other scholars to avoid the turmoil. When in 1978 Belgrade daily *Politika* published an

[30] This is a very accurate assessment of life after the National Self-Defence concept was introduced that strived to mobilize the entire population. Drills of all sorts were not a rare occurrence. I will never forget an episode when, in the early 1980s, phone calls were placed in the middle of the night to SASA members and employees, as well as faculty and staff at the nearby schools (School of Philosophy and School of Philology), so that officials in charge of the Self-Defence programme could measure the time necessary for the employees to assemble. I think that we have collectively failed that test: we all took too long to respond. A large number of us brought books to read and papers to write, none of which were National Self-Defence related.

article and a pamphlet on 'What is Espionage?' (Djordjević, O. 1978: 58, 60) it was alleged that an ambiguous questionnaire was used by American scholars from Columbia University, which had a joint project with the Institute of Social Sciences. Professor Halpern's name was also mentioned and it was implied that his work could potentially be associated with interests outside of purely scientific inquiry. Although his name was soon cleared from the 'slanderous charges' and he was given assurances from ranking Yugoslav officials that he would be always welcome to Yugoslavia, there ensued another unfortunate reaction on the domestic scene. Perhaps as a consequence of negative publicity, a potential deal with a publishing house in Belgrade to translate and publish *A Serbian Village*, for which negotiations had started that same year, fell apart. The book had to wait another twenty-eight years before it was finally published in the Serbian language in 2006 (Halpern 2006), but better late than never!

Was the Serbian side to blame for failed cooperation with American cultural anthropology? It cannot be denied that, when interest was displayed, American anthropologists offered help to their Serbian colleagues. Phillip Mosley used his influence to help Filipović spend his sabbatical in the USA. Halpern, Hammel and others worked very hard to publish a collection of Filipović's papers in America and have his results added to the HRAF. It is true that Halpern tried to establish lasting collegial relationships and collaboration with Serbian scholars in the 1970s and 1980s, but it is also true that, in spite of all good intentions, there have been some misunderstandings and disappointments that have impeded the development of more meaningful and productive professional relationships. While Hammel developed a lucrative collaboration with Croatian ethnologists, which included joint projects, publications and exchange of students (American students conducted field research in Croatia, and Croatian students were offered an opportunity to complete their graduate studies at Berkley – the best of two worlds), Halpern could not boast of such successes. Was he too focused on his own research agenda to notice and support his Serbian colleagues' needs? If this were so, that could have only been a partial answer. More important, in my opinion, was the fact that at the time when Serbian ethnology was ready for a radical change in direction, the interests of its scholars, particularly of younger generation, had considerably diverged from what Halpern was offering. The new generation needed to distance itself from traditional topics and methods of the 'old ethnology' and delve into something entirely different in its quest for its own novel identity. French and British anthropology, functional analyses and structuralism, revisiting old ethnographic materials and reinterpreting them from new theoretical platforms seem to have been more attractive (cf. Kovačević's analysis of the popularity of Van Gennep's *rites de*

passage in Serbian ethnology in the late 1970s and 1980s, 2006: 75–93). Population studies, historic demography, familial and kinship structures, although theoretically and methodologically innovative compared to Serbian attempts in the same area of research, were thematically too reminiscent of traditional topoi to be appealing as a renewed paradigm in Serbian ethnology. This was an unfortunate but logical choice, and another lost opportunity to choose and use, if not the entire package, then at least some elements of the American model.

The Change

Several articles have been written describing the turning point in Serbian ethnology in the mid 1970s and its development since then (Pavković *et al*. 1983; Kovačević 2006: 52–61). I agree with the arguments presented there and I can only offer my personal views and experience as additional comments about the era. In the late 1960s, especially after the 'revolutionary '68', social sciences became attractive not as an instrument of reinforcing the ideology and the position of the regime, but rather as a way to question the system and search for answers and deeper meanings. As a consequence there was also an increase in enrolment at the Department of Ethnology. Among new groups of students there were not only growing numbers of urbanites but also those who had lived abroad for extended periods of time with their families in diplomatic service or technical aid programmes.[31] Fluent in foreign languages, well educated and with personal multicultural experiences, those students have introduced interest in cultures other than their own and revealed different, higher expectations from their studies. However, the response of the faculty and attempts to change the curriculum were considerably lagging behind. Students were thus left with basically two choices: to let their disappointment drive them to pursue other professions or to attempt to self-educate, through literature and/or occasional travel abroad. The socio-cultural context was very favourable. The 'golden '70s' offered not only constant betterment of the standard of living (based more on foreign financial aid than on real economic growth), but also a liberal and stimulating intellectual climate that made educational aspirations possible. However, the real change, or incentive for change, within the discipline came 'from above', when in the early 1970s new faculty members were hired. Bringing in outsiders' fresh views, they managed to challenge the long existing stalemate from the very beginning. For my personal development (just as I was ready to take off and continue my studies abroad), credit goes to the new

[31] I was one of them and I know how much living with my parents in Ghana, West Africa, for five years at a young age, had influenced my choice of profession.

faculty members, and especially Professor Djurdjica Petrović. An ethnologist who had acquired knowledge in history and history of art, skills in using different sources, and who had brought an end to timelessness in studying culture, insisting to the contrary on spatial and temporal precision, did not shy away from comparisons, generalizations and insightful interpretations. Secure in her own domain of research, she possessed one of the most desired qualities a teacher could have: to allow her students the freedom to choose their own paths, as long as they were methodologically and theoretically grounded. Although she had very little experience with relatively recent theories, she had no problem letting me experiment in my masters degree thesis (defended in 1978) with carnival rituals as rites of passage (Van Gennep, E. Leach, V. Turner and later T. Turner and R. Da Matta). In the next phase, while defining the theme of my doctoral thesis, I needed to establish an even greater distance from traditional ethnological topics. Consequently, I chose a subject that belonged to, at that time, the most unpopular domain, material culture and within it styles of clothing, existing mostly, before Professor Petrović began to teach that subject, at a meaningless, unattractive, descriptive level. My goal was to prove that there are no inferior subjects, only inferior approaches, and that all forms of expressive culture are equal in deepening our understanding of culture and society as a whole. Further, I selected to study the socio-cultural aspects of clothing, traditional and fashionable, again in contrast to the 'old' traditional ethnology immersed in the peasant society, in urban areas, notably the capital city, Belgrade, in historical perspective, in the nineteenth and early twentieth centuries. The choice of the timeframe rendered only very limited value to filed research, while the majority of information had to be gathered from various independent contemporary sources (archival documents, the press, photographs, paintings, museum objects and many others) which were overwhelmingly abundant for the period.

The freedom that we were trusted with while struggling to find our own paths, we have granted to our students as well. As a result, Serbian anthropology/ethnology, as redefined in the last quarter of the twentieth century, has developed an incredible diversity of styles and topics, preventing the discipline from falling into the trap of 'movements' or 'schools'.

Epilogue

However, I have never doubted that field research, still a trade-mark of anthropological discipline, if conducted responsibly, is an extremely valuable tool in the study of culture. I am positive that even now when native anthropology in Serbia has become thoroughly 'anthropologized', and when there are lively discussions about the pros and cons of both modern and post-

modern ethnographies, Halpern's 'old monograph' on a Serbian village will continue to be a useful and inspiring source of information of 'how it all once was'. Personally I am very grateful for the exposure I had with the Halperns in the summer of 1978. By then most of us had realized that not only had the 'old ethnology' no future, but that we had no future with it as well. I learned so much that summer about so many things. The most shocking revelation was that I discovered so much about a subculture of my own culture because I saw it through the eyes of American colleagues. At that time, we, the native ethnologists, went on short excursion-style trips to the field. We would usually go in a group, stay in the best available hotel in the local town and radiate from there into villages. We interviewed the villagers in formal settings and everything was highly artificial and stylized. There was a definite distinction between WE, the city scholars, and THEY, the local population. We always went back to our hotel at dinner time. We observed the village life from outside and above and carefully led our informers, not where they wanted to go, but where we needed them to go.

In the summer of 1978 all my fieldwork perceptions changed. I stayed an entire month with the Halperns in their village hosts' family home, far away from the village centre, in a hamlet where the nearest neighbours were either 'down the glen' or 'over the hill'. We spent all twenty-nine days, talking, observing, participating (Carla, Halperns' young daughter, and I managed to lose pigs that were entrusted to our care and then call Mrs. Halpern, by then an experienced 'villager', to help us find them), sharing the good and the bad all day long, from sunrise to sunset. But what a learning experience! First of all I learned how it feels to go through a separation phase from my normal life and live through the liminal time of field research. I learned that one learns the most when not trying to learn but simply sharing chores with the villagers or following them around either on their daily routines, or extraordinary practices like going to the market town, or county fair, or a funeral or celebration of some kind. I learned how specific intentions can be so easily lost in translation. Some years earlier Halpern got fed-up with the chickens ruling the yard and entering the house at their own free will. He thought that it would be nice to have them contained, so he bought chicken coop wire. Our hosts were thrilled with the gift, but they used it in their own, opposite way. They fenced off a small portion of the yard next to the house to keep the chickens away from the dwelling, but allowed them to continue to roam freely beyond the wired area: people were fenced in, while the best quality production of cage-free chickens (and eggs) was maintained. I also learned to observe a bard and decode nuances of meaning imparted by non-verbal communication. I learned to see, by the motion of their heads and eyes, how a person reciting his family genealogy

was using a mental map to help him place generations of his relatives. I learned how important it is to document the process of documenting. For all that and much more, I will be forever grateful to my own special couple of American anthropologists. I felt as if I were lifted from a labyrinth in which I was lost with no hope of finding my way out and enabled to see the pattern from the bird's eye perspective. Later on, I realized that there was also a constant, very intricate pattern of multiple insider–outsider border crossings. And finally, I wish more of us had shared the same experience much earlier. It could have led Serbian ethnology down a different path much sooner.

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PART FOUR

VIEWS AND APPROACHES FROM THE WEST

Chapter 17
Fieldwork in Romania: An interview with David Kideckel

Conducted by Vintilă Mihăilescu

Q: What's an American like you doing in a place like this?

That's a good question. I first came to Romania in 1973. At that time I was a twenty-five year old, relatively new graduate student, just beginning to work on a Ph.D. at the University of Massachusetts in Amherst. When I started graduate school there, I had intended to be an Africanist and my plans were to do fieldwork in Tanzania, in East Africa, studying Ujamaa socialism. This was the policy developed by President Julius Nyerere, where dispersed tribal peoples were to be brought into the nucleated settlements and provided with state support, with state subsidies for education, water, electricity, sanitation etc. etc. And one of the policies of Ujamaa was to collectivize land. In a way, you could say that I was interested in 'collectivization' from the very beginning.

When I got to U-Mass., having known John Cole from my undergraduate days, he invited me to participate in a project that he was organizing in Romania. As you probably know, John had spent much of his career working in Northern Italy, in South Tirol, studying issues of inter-ethnic relations between Germans and Italians. He, together with Eric Wolf, wrote *The Hidden Frontier*. But, as John tells it, he was getting tired of working in Italy where there were too many tourists, and being a leftie himself, he wanted to move further east and find an analogous fieldsite, a mountainous situation, questions of inter-ethnic difficulties, problematic relationships and so on and so forth. He had read about the Braşov region in Romania, which, he thought, could be such a place. He asked me if I would like to participate, join with him in this research project that he was attempting to organize. He had big ambitions, but there was really no one else apart from he and myself at the time – this was in January 1973.

In any case, he got me. At that time, Europe was one of the focuses of U-Mass. anthropology – it might have been the only university in the coun-

try with this European focus. So I took a couple of classes on European history, I don't know, a reading course on socialism maybe … I'm not sure, I can't remember. John encouraged me to apply for a foreign language grant to learn Romanian. So I applied for a grant from the American Council of Learning Societies, Foreign Language Fellowship and was able to secure 1,000 dollars, which for the summer of 1973 paid for me to learn Romanian.

In the interim, while I was applying for this grant, John was organizing his group. Other individuals who agreed to participate included Sam Beck, who was also just beginning graduate school at U-Mass., coming out of the field with his wife Lois Grant, who was a specialist in the Q'asqai Iranian tribal peoples. And Sam had also lived in Eastern Europe. Steve Sampson, who had a Danish wife, Vibeke, I don't know how he got involved in east European studies; Marilyn McArthur, who was interested in Germans, Saxon Germans; Steve Randall who was … I don't know what, mountain people, his thing was mountain people. So there was the five of us and then John. But I was the first one there. I took my thousand bucks and in early July I went to Europe with another friend – a colleague who was going to visit family in Italy and we hung around together for a bit, went to Denmark and Amsterdam and Hamburg and I left them there in Germany. I started making my way to Romania hitchhiking, taking trains and buses and this and that.

Q: When was this?

I got to Bucharest in early July of 1973, knowing only a few words of Romanian. Being 1973, foreigners could still stay with Romanians and I arranged to be put up in an apartment of a woman named Vilma Dumitrela, I remember, who lived down the street from the 'Patria' cinema. So I exchanged with Vilma, she taught me Romanian and I bought her cosmetics, which was her thing – she was very interested in cosmetics. The plan was that I would come to Romania first and that I would be met there later on. I would begin to do the necessary work to set up the ethnographic survey that we were planning for Braşov county, to see potential research sites, village sites, and that Cole and Beck would follow later on during the course of the summer. So I was pretty much alone the first few weeks that I was in Romania; I really didn't know where to look or who to talk to, so I just started looking in the phonebook for various institutions and individuals, professors, trying to be as broad as possible in defining the institutions that I might want to look at. And I happened to run across the Institute of Ethnography at the Academy of Romanian Sciences, where Romulus Vulcănescu was head honcho. I called actually and walked over there and met him – he was very,

very – you know – you could see right away that he was a player and not your typical scholar, but he and I hit it off fairly quickly. And he was very, very helpful. He gave me the most valuable item that I could have possibly imagined at that time: a pocket English–Romanian/Romanian–English dictionary. That was just… it came in so handy.

I also met Mihai Pop at the time, at the Institute of Ethnography, but for some reason, you know, Mihai and I… Katherine Verdery was already there and had clung on to him. And we were close, Pop and I, but I was … you know, I was kind of on the margins. In any case, that first summer there I was just a neophyte.

Before Sam arrived, I decided I wanted to explore the country, and I hitchhiked off to the Danube Delta. I hitchhiked to Tulcea and took the first boat that came to the last stop on the line and that was the Chilia branch and all the way went out to Periprava. I met a Russian guy on the boat, a nice young man … the boat was a lot smaller than it is now, I tell you, and there were peasants on there with chickens and pigs and it was just a good scene and I really got into it. I stayed like two weeks in the Delta, playing soccer, swatting mosquitoes, smoking cigarettes, hanging around with these *Lipoveni* guys, with kids; I didn't speak any Russian and they didn't speak a lot of Romanian but we seemed to get by. This was the kind of fieldwork I dreamed of doing, but I never did!

Anyway, when I came back to Bucharest, Beck had showed up, so I went off with him. We didn't have any official meetings, we were just graduate students, you know, we were tourists essentially – so, Sammy and I went off to the *Brasov* County by ourselves, to scout out various villages. I bought a book in a bookstore in Bucharest *Localităţile judeţului Braşov* (*The Localities of Braşov County*), which kind of specified the diverse villages. I was looking to study collectivization – that was my interest; I've always been interested in socialism and the Russian Revolution and questions of this sort from my youth, for that matter. So I was looking to study collectivization in agriculture. Marilyn, like I said, was interested in ethnic Germans; Randall was interested in mountain communities – as was Sam to a certain extent. Sam was more interested in marginal issues; he ended up doing a study of a frontier zone Poiana Mărului and Sampson ... I don't know what Sampson's specific interests were, I guess central planning. And he ended up doing this whole *sistematizare*[1] analysis. So there wasn't a lot of formal work to prepare for this journey.

[1] 'Sistematizare' refers to the huge project of administrative and urbanism reorganization of villages and cities during Ceauşescu's regime.

Q: You already knew Romanian?

I learned Romanian really by the seat of my pants. From Vilma I took a few lessons, I had a book that I brought from home; she had a list of verb conjugations as well, which was very, very helpful. I had that English–Romanian/ Romanian–English dictionary from Vulcănescu, and then I just went and started learning, talking to people, buying salami, cigarettes, bread, asking directions, etc., etc., trying to read the newspaper, *Scânteia, România Liberă* etc. at the time … *Drum Nou* when I was in Brasov … and that's how I learnt Romanian. I cannot say how the others did, I think everybody did pretty much the same.

Q: Let me come back to my first question: what brought you here, in fact? Was it just chance or was it something more?

Yes and no. All of us had different motivations and were at different places in our careers. To me there was a certain political interest in working in a Cold War country. We talked about being interested in the … in lived socialism and that was a concept I think that developed all along, little by little as we worked on this project, it wasn't the abiding thing at the start of it. I was always interested in communism and Russia and the Russian Revolution and Trotsky and so on and so forth, so I got a particular kick out of thinking of working in Romania. I had no, at least I didn't think that I had any, personal ties to Romania when I started working there but just recently, however, I found out that my grandmother was from Bucovina, but the Ukrainian side … she spoke Romanian. I know that they said that she spoke seven languages, but that's a different story. So that's that.

Q: How did you choose your fieldwork?

Well, being a kid from Detroit, a gritty industrial town, at the same time I also spent a lot of time outdoors as a youth, camping and canoeing and so on and so I was kind of interested in industrial issues. My family is a union family, my uncle was a union organizer in the early days of the United Auto Workers, and so was beaten up by a squad of goons hired by Henry Ford in the organization of the UAW, so I was always predisposed to these kinds of issues. And as a consequence, you know, in the midst of the Cold War and going to Romania and all or even before that, thinking about Tanzania, I was interested in rural socialism and so as a matter of course I gravitated to wanting to study the collectivization of agriculture. I was also operating under some kind of anthropological imperative, how typical American

anthropologists very often think they want to get far away, be original and I was hoping to end up at a distant collectivized community. I was thinking first of Mărgineni, I saw on the map from that book, *Localităţile judeţului Braşov*. So I planned to go out there with Sam. John wanted to be in a town near Făgăraş, centrally located.

We were all going back in January 1974, the six of us, sponsored now by the University of Massachusetts, Anthropology Department, European Field Studies Programme. And every year, in the spring semester, a faculty member would take a group of students to Europe. Usually, the students were dispersed all over Europe, in different countries and the faculty member tries to keep track of them all. I think John and the rest of us were the model for this when a faculty member would go over with a group of cooperating students, and so would all work in the same area. We certainly had a common stake in choosing our topics, since the whole thing, the conception of the thing was to do this group project in Brasov County. We were going to distribute ourselves both according to geography, so Randall was going to be in the far uplands, Sam a little bit further down, me on the Romanian plains, John in a small suburban community as it were, more rural and then Marilyn and Sampson in Ţara Bârsei, Marilyn with Saxons and Sampson with the larger urban kinds of issues. If there was any kind of hesitation it was on my part, since after spending that time in the Delta I thought that I would really like to go there. But John kind of put the hold on me as it were and pushed me a little bit and encouraged me to do the work in Hărsenî. I finally decided to work in Hărsenî.

Q: Why?

Because of an incident. When Sam and I were looking for villages in and around Făgăraş, we had stopped at the gates of the *Combinat* (the chemical enterprises of Făgăraş), there. We were just hanging around, waiting for a bus which we figured would take us towards Mărgineni when we met two guys who were coming out of their shift at the *Combinat*, a man and his son-in-law. Turns out the guy was a *pocăit*,[2] you know… typically marginal. So he and his son-in-law struck up a conversation with us and he invited me back to his place, says 'Come on, come to Hărsenî and stay with us.' I remember it was still the summer of 1973, and you could do that. And so we got on the bus and we went to Hărsenî and we stayed with this guy, Rinea was his name, Preoteşu his nickname, now dead as most of these people are. We stayed with him for two nights I think, and his son-in-law took us on a

[2] A neo-protestant church.

tour of the village and we visited some of their other friends and we saw a whole lot and they were very forthcoming. They fed us well and I told him I said okay, you know, I'm going to be back in six months. So that's another reason why I kind of hit on Hărsenî, I had an 'in' there, it was also the residence village of the 'commune' and that would give me greater access to village records and so on and so forth. Because as the project evolved, at least my idea of it, it became something to look at the inter-relationship between the village and its social relations and values and relations with the state and seeing the CAP as a mediating form between the two of them. Also, of course, it was different from the others and was going to fit into this larger concept of Cole's of socialist development in one region. And with the development aspect, that's how I got interested in industry as well, because that was the interesting thing about the place, it was a place that was trying to develop collectivization at the same time that the state was making all these industrial demands on it.

Q: Who financed your research?

In 1974, the six of us were there supported by U-Mass. on this European Field Study Research Programme. But our major field research was done in 1975 and 1976, at which time we all received IREX grants and Fulbright grants as well. Now, I don't know if all of our grant proposals were fundable but I'm sure they weren't gonna deny one of us and accept the other four for example, so in that sense it was good to be in a group, apply as a group. IREX was a great grant at that time because it was not an official government to government organization like the Fulbright grant was, but IREX had a research exchange relationship with CNST, *Consiliul Naţional pentru Ştiinţă şi Tehnologie* (the National Council for Science and Technology) of which, at that time, until her untimely death in December of 1989, Mrs. Ceauşescu was the head. So we had great connections as it were and it was that IREX–CNST link that enabled us to get into the field. In 1975 now, when we came, already we needed special permission to do fieldwork because in 1974 the Official Secrets Act was passed and it was now illegal for Romanians to speak with Americans without some kind of subsequent police report. We needed to get beyond that, we needed official permission to stay in people's homes, and that took a while… we got to Romania and it took a while for each of us to negotiate our ability to get out into our various villages. I didn't really realize it, when I selected Hărsenî and the Făgăraş area to do research, but Făgăraş, of course, with the *Combinatul Chimic Făgăraş* there, was a centre of Romania's munitions industry and it was a highly sensitive field site, the whole area was in fact… at Victoria there was an

explosives factory, in Mârza there was an ammunitions factory… so the people in the state thought I was really doing some kind of sensitive research.

Q: What about your contacts and relations with the Romanian colleagues?

My initial relationship with Romanian academics was somewhat ... I wouldn't exactly say standoffish … but I was a little bit sceptical and suspicious. Certainly there was an air of threat or uncertainty in the environment in Romania in the mid-to late-1970s and even if people – academics – averred their degree of independence from the Party and from official Romanian institutions and *Securitate* and the like, one always had that in mind, that you always asked this question: is this person befriending me for purposes of befriending me or is this person befriending me to keep tabs on me; or is this person befriending me and relating to me for instrumental reasons? I'd recognize the hunger for information that people had and in that sense I tried to be a good colleague, bringing books or copies when back from the States, and so on, but I have to say, you know, I kept people to a certain extent at arm's length and I did not feel it incumbent upon me to be involved in Romanian academic debates or to enable Romanian academics to be too close to me.

Another issue that kept my relationship from developing with Romanian scholars as fully as possible was that I kind of felt I fell through the cracks in terms of Romanian intellectual practices. The institute of ethnography that Mihai Pop headed up became even more wedded – at least in a formal way – to the party and the State when it was changed to the Institute of Ethnography and Dialectology. The sociologists, on the other hand, engaged in a rather extreme positivistic type of sociology which I felt not uncomfortable with exactly but certainly not at home. Their concerns – except for few exceptions such as Michael Cernea and Trăian Herseni – were mainly to particular studies of industrial factories, of commuting youths, like Ovidiu Bădina's work on "Youth Today" and so on and so forth … and so I didn't feel so very much at home in the theoretical sense or even in an ethnographic sense since then … So, as a young graduate student, in the mid 1970s, beginning work in '73, with six months of field research in '74 and then again in the year and a half in '75–6, I did not have such intense, enduring, constantly cooperative communicative relationships with Romanian academics.

On the other hand, the kind of long term field approach I had was a different style of operation, as Tamás Hofer observes in his article. So that people were even wondering about and were sceptical about my interest in

and concern about village life and the fact that I indicated that I tended to enjoy life in the village and preferred not to bother to return to the cities so very often.

Q: Weren't there people to 'take care' of you, to have you in their charge?

The IREX grant stipulated that we had to have an *îndrumător*, a guide, and they selected for me Constantinescu, Virgil. I would have worked with Cernea, but Cernea upped and left in 1974–5, when he got his initial position at the World Bank, so he wasn't really available to be my *îndrumător* as such. Constantinescu was a typical positivist sociologist working in the Institute of Sociology, headed by Ion Drăgan. Members of that Institute also included Maria Fulea, Maria Ciobanu-Băcanu; I think Maria Larionescu was also there, Nicolae Gheorghe and Drăgan himself. There were rumours that the Institute of Sociology was somewhat independent from party procedures and processes, and in fact Drăgan had moved the Institute to a street that was just off *Piața Rosetti*, near *Hotel Banat*, or that area there. And the move was alleged by some to represent an attempt by the Institute to remove themselves from a greater degree of control from Party oversight and institutional surveillance. These were rumours and, like I say, there was always a degree of suspicion on my part as to the purposes of individuals and relationship to myself.

Constantinescu and I barely had much of a relationship. I would visit periodically, I would come to Bucharest on a monthly basis, in order to pick up my stipend from Mr. Beiu at the CNST, the National Council for Science and Technology and typically in these monthly visits I would also stop by the Drăgan Institute, making appointments with Constantinescu, to let him know how my work was going. I was not expected to file any papers or anything of the sort, which was nice, and at the same time I didn't receive a particularly great amount of advice or direction from Drăgan, Constantinescu or anyone in the institute. If anything, I have a sense that Constantinescu was as fearful of me as I was suspicious or uncertain of them. To a certain extent, having a relationship with an American academic was problematic at the time and people very often would rather not, if only just to cover their rear ends; it was simpler for them and I could certainly understand that. So we had a reasonable relationship, I would see them, I would talk about what I was doing, but it didn't go further beyond that.

Q: Did you talk about your research with Romanian colleagues?

We got to Bucharest in 1974, that's when we got talking to people and institutions. We gave a talk at the Academia, where all these sociologists and political scientist types were, and the talks were on the nature of our research methodologies and questions. This was a large meeting actually, if I remember correctly, in early 1974, where the six of us – John of course, myself, Sampson, Sam, Marilyn and Randall sat on a large stage and presented a discussion of our work. Basically, each of us was asked to indicate what our topic was and how we were to go about studying it, what methods we intended to use, the kinds of people that we hoped to interview, other kinds of data that we were seeking. Certainly there was a two-fold purpose here. One was for some degree of scholarly exchange between ourselves and the Romanian academic community, and to that extent there were a large number of individuals there who were actually *bona fide* sociologists, social scientists, Cernea for example was at that meeting, I remember, but there were also a great number of individuals whom I did not know, in cheap suits, and who were clearly members of the party establishment and who were, you know, looking us over and listening very clearly to what it was we were about.

Q: Beyond these rather formal contacts, didn't you have also some 'real' academic relations?

My most important relationships were those that were informal, with Cernea and Herseni, both of whom I visited frequently, whenever I could when I was in Bucharest and had rather lengthy talks with the two of them. Especially with Herseni, because Cernea at the time was beginning to get involved with his work at the World Bank where he ultimately ended up. On the other hand, Herseni was old, infirm, at home, and was rarely visited by colleagues – in fact he would complain that he tended to be ignored by many, perhaps of course because he was a political liability – and so I think he looked on visits with me as somehow gratifying and enjoyable. And then, there was Nicolae Gheorghe, the Romany sociologist in Dragan's institute. It was with him that I would have lengthy and intense discussions about the nature of Romanian socialism, about socialism in general, about aspects of the Party and collectivization. I mean I felt fairly able to be open with Nicu in a way that I couldn't even with people like Vulcănescu and certainly Mihai Pop. I know what Verdery and Kligman say and think about him and I also understand Pop's attempt to maintain political independence, but even

to this day I find and feel a certain degree of uncertainty about who Mihai Pop was both in a political and an academic sense.

Q: Have you been watched by the *Securitate*?

I do know that I was surveilled by the some of the individuals with whom I worked, both Nicu and Vulcănescu admitted as much to me and both of them did so in typical, almost caricatured, 'spy versus spy' kind of ways. I remember Nicu saying that he had something important to talk to me about and he just didn't want to talk about it in the apartment, implying, of course, that the apartment was bugged. So we had to go across the street into *Parcul Tineretului* when it was dark, and continued to walk and so he would then admit that the *Securitate* had come to him and demanded that he write a lengthy written report about what he knew about me and my relationship with him and the kinds of work that I was doing. Vulcănescu too took me in his car and we drove outside of Bucharest one day and he said 'just listen'; he really wanted to tell me to be careful, that people were watching me and that they knew who I was. I remember how he particularly said, how they were watching me stand at the corner and talk to the commuters as they were going, leaving Hârsenî to go to the factory and that they were watching me count the number of commuters, which actually I really never did.

So these kind of things always kept one off balance and always made one sceptical about getting too closely involved in academic life and besides, one could see the lay of the land in the mid 1970s, and the extreme development of the cult of personality, the bastardization of science under Mrs. Ceauşescu and she headed the CNST, the destruction, if you will, of liberal arts and the wholesale shifting of scientific practice to serve the needs of what was called the productive capacities of society. It would almost be nonsensical; at least I thought so, to be involved with, in a heavy duty way, large scale theoretical debates in Romanian society. There was no rhyme or reason to it.

Q: Let's go back then to the years of your research. What was it like to do fieldwork in this context?

Doing fieldwork was absolutely rewarding. I loved it. I had a great time.

Q: Professionally or politically?

Professionally. Politically no. I mean anybody that showed any sympathy with one's purported enemies wasn't going to go very far in the political

sense in the USA. Also, as things got worse in Romania through the 1970s, into the 1980s, I began to be criticized by my colleagues at home for having bothered to maintain a relationship with Ceauşescu's Romania. But it certainly was good to be different. It wasn't really the greatest thing to do to ensure a job appointment because American anthropology then and American anthropology now is very biased towards 'indigenous type societies', Africa, native North America, Latin America etc. And Europe, even though it's achieved a good degree of credibility, theoretically and all, is still really not on peoples' radars in terms of hiring, so it wasn't the greatest choice as far as that goes. At the same time, it wasn't a very smart choice to work in a group. Though it was interesting and it was a good experience, at the same time think about it: to have five people coming on the job market at the same time with roughly the same skills and the same backgrounds… you kind of put yourself at a disadvantage in that way. But nobody wanted to think about those things at the time, the opportunity presented itself, the money was there, the support was there and so one did it.

Q: Was fieldwork in Romania different from other places?

Well, it's hard to say because I hadn't really done fieldwork in other places. It was pretty much what I expected it to be, villages everywhere are kind of quiet, sleepy places; I thought that it would be more of a public life related to the collective farm and the like, I had good friends, young intellectuals mostly. But what can I say, there was a lot of instrumentality in the field situation. People were looking for you to do things for them in socialism, given the economy of shortage. There were always requests for this favour and that favour and these material goods and those material goods … people weren't too obnoxious about it and I really provided bits and pieces only to those who were really close to me.

Q: What were your personal affinities, methodological and intellectual ones?

Well, the folklorists, I found interesting people like Vulcănescu and Pop. To a certain extent, I was a little uncomfortable between the two of them because every time I was with one, the one would talk about the other. At the same time folklore was undergoing some heavy duty transformation at that time, in the mid seventies when it was becoming the Institute of Dialectology, and there were all kinds of uncomfortable political rumours going around about the folklore establishment. I've also always been uncomfortable with the concept of *tradition* and to a certain extent most of the folklorists beyond Pop were very much oriented to that European kind of folklore,

collecting, cataloguing myths and masks and other kinds of habits. And we can see there wasn't much of a cultural anthropological approach in folklore. Herseni, his book on the *Ceata feciorilor*, I thought an interesting example of a cross between a sociological and anthropological approach using this folkloric form, if you want to call it that, as the basis for an extended meditation on village social relationships and the like. That was fairly astute.

There wasn't that much in sociology either for that matter, as to my mind the sociologists wern't particularly interested in social processes, conflicts, real life as it were. Certainly Romanian sociology at the time had its great shortcomings. It was heavily political, the articles were rife with this *limbă de lemn* (wooden language), and they mainly dealt with themes like Bădina's work on youth, that were in the interest of the party. I have to say, Cernea's book on the collectivization of agriculture was, I think, in keeping with a Western kind of approach. It certainly was statistical and examined the CAP (agricultural cooperatives) in a macro sociological, organizational kind of sense, but one did get a feeling from that and from his other book *Două sate*, that he wrote about the specifics of life on the ground, of how real people live under real conditions. There wasn't a lot of concern for meaning in their work, certainly they were much more organizational in their approach but nonetheless I found a much greater degree of affinity there.

I argued all the time, though, with almost anyone, about various issues with which I didn't agree. I particularly pointed out the contradictions of Party control of the CAP. Once I was involved in things… people were really, at that time, except for a very, very few, in a very few circumstances, people were really skittish of being too too closely involved, I think, with us. And most certainly weren't coming out and talking about the difficulties or the extreme problems of the system. They would phrase their appreciation or lack thereof of Ceauşescu and *Partidul* (the Party) in very guided… not guided… very covered over kinds of ways. I thought in fact Romanian social science at the time was rather stultifying, was kind of in a deep freeze. It was there, but it was so *pro forma*, I mean most of the sociological works – Trebici was good, I thought, his demographic stuff was pretty interesting; Cernea like I say … But by and large, the vast majority of work being produced in Romanian sociology, the journal *Sociologia Românească*, and the like was really kind of, so overly statistical, focused on rather narrow problems. It didn't really deal much with the political realm at all in fact if only just to support party policy in so far as the rural social change was concerned, that's what I was particularly interested in.

But I wasn't shy about criticizing the system, which I did, you know, in measured kinds of ways. I also indicated that I recognized – and still do in fact – some of the positive aspects of socialism. But I criticized it in face-to-

face meetings with, like I say, my colleagues. Nicu Gheorghe was pretty good about this; he was very clearly of a different sort. I got to know Nicu because he was a researcher at the Drăgan institute; this was before he came out as it were as a Roma activist, although he did, towards the latter part of my fieldwork. Nicu was remarkably well read and we spent a lot of time talking about social theory. More so, I think, he was actually more on top of this game than virtually anybody I met in Romania. And still today I am just amazed at the extent to which he was able to secure access to information, latest articles, books and so on and so forth.

I also tried to present my work – I did present my work – to various individuals in the local community. I spoke frequently with the *primar* (Mayor). In 1979, actually, when I was back for six months, there was a convocation sponsored by the local committee of I don't know what, Socialist Education or something like this, that was organized by my friend. At this session I wrote up a four-page typewritten presentation. I asked my friend if I could be put on the programme since I had arrived rather late. He allowed it and I wrote up a four-page presentation about the problems with collective farming, local collective farming, in particular the conflict over industrial and agricultural labour that was being experienced by the households and issues of monetization. However, at that time, getting towards the 1980s, with the introduction of monetization, where people were no longer paid in kind for their work on the CAP but received money instead, my paper was the one thing that was absolutely not needed, especially in those communities that were so heavily involved in industrial production in Făgăraş. They were not happy, I can tell you!

So, to sum up, Romanian social sciences in the mid 1970s were very much in thrall of the political system and were kind of at an impasse as it were. There were so many bright people, so many interesting people, people who were looking to develop relationships with Westerners and others, looking to participate in global intellectual networks and who were frankly unable to and were fearful of it at the same time.
Theoretically I had little if any respect for Romanian social science…

Q: How would you sum up the differences between American and Romanian social sciences – or approaches to social science?

In so far as the main differences of approach between Romania and America, concerning social sciences, I would have to say that Romania, to a much greater extent, has kind of a *nationalist project* behind much of its social science, that includes the best and the worst. It is very much oriented to or at least has in mind certain kinds of national issues, national goals, national

histories and questions in its approach – that's one basic difference, whereas American anthropology, social sciences and the like, tend to be much more universal in their orientation. Certainly, this is a function of the size of the two countries and their position in the international community too, but that being said, that is still a major difference. I also think that there tends to be a greater degree of *formalism* in Romanian social scientific approaches and less a concern for *process* and the give and take of social relationships. Romanian social science still has a very strong *institutional focus*, as opposed to a personal focus. The individual tends to disappear in Romanian social science whereas individuals even in sociology in the United States have been once again brought to the centre stage. Nonetheless, Romanian sociology, social science still has many solid… it has great traditions to call on, it is very, very self conscious, and in some ways it is not so… even though is self conscious, it is not so solipsistic, ego-centric as American social science is; reflexivity, as it were, never penetrated into Romanian social sciences and if you ask me that's probably a good thing. Americans tend to be very fetishistic and we kind of shift directions like flocks of geese in the air, you know; when there's one individual or a few individuals that developed some kind of theoretical significance, explanatory power and the next thing you know is all these geese are going off in that direction and then they'll come back in the next direction. To a certain extent, Romanian social science, both positively and negatively, has remained on a more consistent path. Both of us suffer from the same kind of petty jealousies, though in some senses the American situation is much more wide-open giving an individual who suffers from such petty injustices a chance to develop and move away from them, whereas Romania being the small system that it is… Such is the situation, it seems to me, with anthropology in Romania now.

Chapter 18
Fieldwork in Bulgaria: An interview with Carol Silverman

Conducted by Chris Hann

Q: Could you explain how you came to start your project in 1970s Bulgaria?

I began research in Bulgaria in the 1970s as an undergraduate because I was a devotee of Bulgarian folk music. I think I may be unusual among anthropologists and folklorists in that I took my hobby and made it my career. I first travelled to Bulgaria as a collector, a singer and a dancer, and then I went to graduate school to acquire the analytical skills to analyze the folklore materials I was already studying. At festivals I would seek out singers whom I had heard on recordings, and often they would generously invite me home to their villages. I was often the first westerner to visit these remote locations. In those early days I had many naïve and romantic concepts about village life—about artistry, harmony and beauty, but I quickly learned that conflict and politics infused every art form in every locality. Many of my first trips were to Pomak (Bulgarian Muslim) villages where the Muslim population was experiencing mandatory name changes and prohibition of their music, costume and culture, as well as political repression. I hadn't planned to study the role of the socialist state, but the state was everywhere, so I took careful note and formulated my research to explore what the state endorsed, and what it censured and why.

At the University of Pennsylvania I did not have a mentor who specialized in the Balkans, so I developed my own research questions based on what I observed and documented in my trips. I first focused on what became official folklore and what was omitted. I thus looked at socialist state cultural policy at a time when it was extremely sensitive. My 1983 *Anthropological Quarterly* article 'The Politics of Folklore in Bulgaria', showed how nationalism and socialism affected forms of state-sponsored culture such as music festivals, ensembles and schools, and in collection activities. Soon after I began studying how people (especially minorities) resisted state categories and developed unofficial forms of expression.

Q: Given that you had no mentor specializing in the Balkans, did you perhaps find any inspiration among Bulgarian scholars? How were the boundaries drawn at the time between ethnography and folklore, and where did ethnomusicology fit in?

My research bridged the disciplines of folklore, ethnography and ethnomusicology, so I intersected with three different institutes. As explained by the Bulgarian authors in this volume, ethnography was considered a historical science and the fundamental concept was the Russian idea of ethnos. The aim of ethnography was to study the culture of the Bulgarian people, and this was assumed to be identical to the Bulgarian nation/state. The focus was on peasants as hard-working people and as the true bearers of traditional culture. Imagined as industrious and stoic, the nation was pictured as withstanding the onslaught of outside forces. This narrow vision ignored all minorities and all neighbouring nations, although paradoxically it valorized, at least rhetorically the 'Slavic brotherhood' of nations, led by Russia.

Ethnography sought to explain the ethnogenesis of customs and way of life (*bit*). Bit was defined as the material life of peasants. The Institute of Ethnography was located in the ethnographic museum, and its researchers often used the superb collections housed there, including costumes, house types, work implements, etc. I was also interested in costume and spent many hours with the exhibits and behind the scenes. The minutiae of peasant life was collected and catalogued; collection and cataloguing were themselves legitimate goals. The study of bit was often combined with the study of spiritual culture (customs) in many volumes labelled *Bit i Kultura*. Materials were organized by region; the nation provided the framework but within it, divisions were above all geographical. Fieldwork expeditions were organized by regions, scholars became specialists in regions, and collections, publications and exhibits were also organized by region. Ethnicity, on the other hand, was routinely ignored because the ethnic minorities were problematic for the model of the homogeneous nation. Gender was similarly ignored.

The Institute of Folklore conformed to the same ideological rules but intellectually it was inspired by literature and was thus concerned with verbal art, with variation and creativity and with the folk artist, whether singer or storyteller. Perhaps because it was relatively new, The Institute of Folklore seemed to be a place of lively discussion and a young group of scholars, including many women, worked there. My strongest early ties were with folklorists because I was interested in ritual and song texts. The director of the Institute of Folklore, Todor Ivanov Živkov, welcomed me cordially. In spite of one or two published portraits of individual folk artists, most folk-

lore was assumed to be located in groups. I was more interested in individual life choices regarding artistry, a topic not addressed in the Bulgarian literature. Nevertheless, I benefited from conversations with Radost Ivanova, Georg Kraev, Ana Ilieva and many others. I admired Ivanova's book *Bălgarska Folklorna Svatba* (1984), a thorough ethnography of wedding ritual based on many years of fieldwork; when it was translated into English I used it in my university classes. But I was disappointed that it did not include anything on contemporary weddings. I became friendly with Ivanova's family and later sponsored her son to study at the University of Oregon.

Ethnomusicologists were housed in the Institute of Music, and they dealt with the sound aspect of music; they too collected and transcribed. Since my husband Mark Levy is an ethnomusicologist, and we often did research together, it was necessary for us to maintain ties to that institute even though the focus and methodology seemed artificially narrow. Coming from the highly interdisciplinary field of folklore in the United States, I thought it was inefficient and unproductive to divide culture into these fragments. When studying folk music at a wedding I had to read folklorists to learn about the song texts, ethnomusicologists to learn about the melodies, and ethnographers to learn about the customs. Of course these divisions, though upheld in most publications, were not rigidly respected in individual researchers' minds; in truth many researchers were knowledgeable across the cultural spectrum. I had inspiring conversations with ethnomusicologists Dimitrina Kaufman, Vergilij Atanasov and Cenka Jordanova, for example, about contemporary wedding rituals.

Q: Did this methodology have any advantages, to compensate for its narrowness?

Well, I was dazzled by the encyclopaedic knowledge of folkloric materials that my colleagues had at their fingertips. The sheer volume of collected materials was overwhelming; I pored over song collections and was amazed by the time put into classifying collected texts. In hindsight I am very glad that I gained an appreciation of the painstaking collection work of my Bulgarian colleagues. Furthermore, I believe that immersion into the materials of folklore and ethnography is necessary before theorizing about them. I have seen too many non-Bulgarian ethnographers generalize too glibly about Bulgaria without a deep knowledge of ethnographic materials.

In spite of the drawbacks of socialist paradigms I was inspired by the devotion of the researchers, many of whom really loved their work and helped me unofficially in any way they could. Some researchers did their work with verve and intellectual excitement—others seemed to be bored.

Naturally, I gravitated toward those who were truly devoted. I found that several publications of the early socialist era were superb examples of creative contextual and historical ethnography—these included Hristo Vakarelski's ethnography of the town Momina Klisura and Raina Kacarova's articles on the life history of a female musician. Kacarova dared to deal with topics such as gender and ethnicity when it was radical to do so.

Q: What about fieldwork?

In spite of my admiration for the depth of knowledge exhibited by my Bulgarian colleagues, I was frustrated because most could not fully understand the American fieldwork methodology of participant observation. Whereas they typically made short trips to villages and sought out elderly informants from whom they elicited texts and memories of an earlier era via questionnaires and surveys, I wanted to live in a place for a long period of time and observe, document and participate in contemporary ritual and musical life.

There were several problems with my goals. First of all, I could not obtain permission to live in a village, especially a village with a sizeable minority population or a village too close to the border. My husband faced similar challenges—in 1979–80 we had to formally agree to live in the town of Smoljan in the Rodopa mountains and travel daily to remote villages, wasting precious time. Of course, we ignored the rules and regularly slept in villages. We also had to agree to have a member of the Institute of Music with us every time we filmed a musical event; we ignored this rule when we realized that no one was enforcing it. All of these prohibitions were predicated on the assumption that we should not be allowed to roam around unsupervised, especially in multi-ethnic regions; the authorities were suspicious that we were spies, troublemakers or journalists. I believe Gerald Creed was the first western scholar to receive permission to live in a village, and that was due to some very special links he was able to forge with rural sociologists.

Q: I recall ethnographers in Hungary who used to argue that they did not need to do fieldwork in the way we did, since they were immersed from childhood in the culture in a way that the foreigner could never hope to emulate. What do you make of such arguments?

It is quite true that almost all of the researchers had relatives in villages; like most urban professionals, they were just one generation away from peasantry so they had experience with village life. Some had deliberately distanced themselves from peasantry in order to establish their authority as scholars;

these researchers even resented spending the night in a village. On the other hand, others genuinely appreciated and admired village life and culture. Peasantry, then, was alternately disparaged and exalted.

Q: Again, if I think back to my work in Hungary at the same period, I sometimes argued for a division of labour between the local and foreign scholars. Working mostly on issues of politics and economic development, some of them ideologically sensitive, it was possible for me to publish accounts that would not have been possible for those pursuing careers in their own country. Do you think that your Bulgarian colleagues would have gladly endorsed your methods and your critical perspectives on socialist cultural politics if they had had the freedom to do so?

No, I think that most if not all ethnographers simply did not understand my focus on contemporary cultural processes; they were focused on the 'classic village', an artificial construction that presumed stasis and isolation. As I said, the village was presumed to be the historical locus of Slavic culture and the bastion of peasant folklore. Rural folklore was seen as the defining site of nationalist pride, and it provided the evidence that Bulgaria had resisted 500 years of Ottoman domination. Many researchers sought to prove that folklore was a means to repel foreign influence, inspiring the fight for independence. In this case, 'foreign' was a gloss for 'Turkish', thus, routinely researchers had to make the absurd claim that there were no traces of Turkish music in Bulgarian folk music; it had remained 'pure' and 'authentic'. Some actually believed this claim; their belief may be somewhat understandable because they were not allowed to study Turkish music. This nationalist configuration plagued all the ethnographic disciplines.

Q: But how did this materialism interact with socialism and with Marxist-Leninist ideology?

Ethnography and the entire theory of *bit* were based in Marxist materialism and of course, it was mandatory to extol the socialist consciousness of folklore. Folk songs were seen as demonstrating the revolutionary spirit of the nation, and partisan and workers' songs were given special attention. Religious cultural material was especially problematic for analysis because religion was seen as an archaic survival from a feudal era. It is not surprising that many ethnographers were recruited by the state to develop new socialist rituals (such as the secular wedding ceremony, the secular funeral and the soldier induction ceremony) that theoretically served to replace traditional rituals. In reality, traditional religious rituals existed side by side with con-

temporary ones; whereas researchers were obliged to ignore the religious ones, I focused precisely on their juxtaposition to contemporary rituals.

Q: Was there any room here to make comparisons with neighbouring countries?

I was constantly surprised at how little my colleagues knew about other Balkan and even Slavic cultures; some honestly believed that every aspect of Bulgarian culture was unique to Bulgaria. Others, however, were hungry for comparative materials but could not easily obtain them. Because I was simultaneously travelling to Yugoslavia at the time, I often brought books and articles on comparative Balkan materials as gifts to scholars. We must also remember, however, that the other socialist Balkan nations had similar nationalist ideologies; comparative analysis was absent there too. I honoured requests from my colleagues to bring them anthropological books from the west that were unavailable in Bulgaria.

Q: How did the rising tide of nationalism in the 1980s affect you as an American, particularly since you were working with a non-Bulgarian minority?

In the 1980s I did research on wedding music, a fusion genre influenced by both the west and the east; it combined rock and jazz with Turkish and Romani music and was created mainly by Roma. I did not pick this topic in advance; it simply demanded my attention because it was the most popular and controversial music in the 1970s and 1980s. If one were travelling around villages at the time one could not help but notice the craze for wedding music especially among the youth; people would travel hundreds of miles to crash weddings to listen to live performances of the wedding music stars; and their bootleg recordings cost a month's salary.

I too became a fan, met the stars and their families, and documented dozens of weddings. All of this research was unofficial because wedding music was prohibited by the state for being impure, kitsch and containing foreign elements. As the Bulgarian ethnomusicologists wrote polemical attacks against wedding music and the state jailed the stars to set an example, the musicians became countercultural heroes. I thus studied resistance to the state in musical, artistic and economic terms. In addition to bans against circumcisions, and Turkish and Romani music language and costume, the state embarked on a name-changing campaign against Muslims with violent consequences. The atmosphere in academia in the 1980s became very tense as this 'revival' process was carried out.

Q: How did your Bulgarian colleagues react to these interests of yours?

Some researchers at the Institutes knew of my interest in wedding music but they neither helped me officially nor sponsored me; they simply could not. By the mid 1980s Roma did not officially exist in Bulgaria so there was no way to apply for permission to study them. If I should bring up Roma, they might say they didn't exist but then wink. I did all my fieldwork unofficially on tourist visas and basically hid my research on Roma because of the charged nature of the topic. In 1985 I received a small grant from the *Centăr za Bălgaristika* where I had a 'cover' archival project but actually went to Romani weddings at weekends for my research.

I was not censured by any researcher because I stayed away from the Institutes at this time, but on several occasions I verbally sparred with Todor Ivanov Živkov because he defended the name-changing campaigns. I also disagreed openly with ethnomusicologist Manol Todorov, because he was a vocal critic of wedding music and a defender of state regulation. I remember sparring with Todorov when I attended a university class that he was teaching. When he said that foreign elements were ruining wedding music, I countered that they were creative innovations. Although I had been invited by him to be a guest at this lecture, I felt I had to speak out so that the students could hear an opposing view. As a foreigner I had the luxury of being able to do research unofficially and then leave; those Bulgarian researchers who were critical had to confront state policies directly and continually. Ethnomusicologist Lozanka Pejčeva, for example, had to pick a substitute Slavic folk music topic to investigate because she was not allowed to study Roma; similarly, ethnographer Elena Marushiakova had to go to Slovakia to study Roma because at home she could not. I also regularly heard of colleagues who were denied promotions and other perks such as fieldwork funding because they disagreed with state policy. Several female researchers were able to emigrate to America via marriage; Ljubomira Parpulova, for example, continued her career in folklore at Ohio State University.

In short, the conditions under which colleagues worked in the early and mid 1980s seemed to me worse than the 1970s, due to repression and repercussions of the 'revival' process. On the other hand, the entire period suffered from totalitarian supervision and ideological rigidity.

Q: Was there not a little softening in the last years of the regime?

It is true that in the late 1980s the state loosened up a little in the realm of culture; for example, state ideologists reversed their criticism of wedding music and began to embrace it by organizing festivals as a means of control.

At this point, several scholars such as Dimitrina Kaufman wrote informative articles about the historical roots of wedding music, and they could more openly engage in dialogue with me. But even then, some researchers were never quite sure of what they were allowed to say. I remember that I received sharp criticism from a visiting Bulgarian linguist after a lecture I delivered at University of California, Los Angeles in the late 1980s on repression against Muslim Roma and their cultural resistance to it. Publicly this scholar still felt obliged to defend state policy but privately she told me later that she agreed with what I said; moreover, as a member of another minority, she herself has suffered from the repression of the state.

During the protests that lead to the downfall of the communist party in early 1989 folklorists and ethnographers did some pioneering work. They documented the politics of protest in the 'city of truth', a sea of tents that was erected in the centre of Sofia and served as headquarters for the opposition. After 1989, ethnographers quickly opened up to western methodologies and theories but it has not been easy to unlearn propagandist assumptions. I have been impressed by the newer generation of Bulgarian ethnographers, ethnomusicologists and folklorists who have questioned some of the foundational approaches of their training. I have taught some of them at the University of Oregon and have witnessed their flowering when exposed to new theoretical frameworks and comparative Balkan materials.

Q: Asen Balikci has written critically about socialist era ethnography in Bulgaria. What is your overall view of that legacy?

I agree with Balikci that the greatest strengths of socialist ethnography were its voluminous collections and thorough descriptive record. Yet this rich record highlights a gaping hole—the absence of documentation of minorities. Thus I also agree with Balicki that the nationalistic orientation of ethnography in the socialist era was its greatest drawback. Not only did nationalism stifle discussion and support political repression, but it also prohibited research on the diverse cultures of the ethnic minorities and their interplay with Slavic cultures. I still feel deep regret about the significant work that could have been done by Bulgarian colleagues during the socialist period to analyze the mosaic of cultures that makes up Bulgaria. Thankfully, in the last two decades this gap is finally being addressed. The introduction of new theoretical paradigms and new methodologies is revitalizing the field of ethnography.

Chapter 19
Fieldwork in Serbia: An interview with Joel M. Halpern

Conducted by Mirjana Prošić Dvornić

October 2003 to April 2004 (Revised in January 2007)[1]

Fifty Years Later

Q: In 1947 you left your native New York state to attend the University of Michigan in Ann Arbor. You intended to study chemistry but got a degree in history. What happened?

In fact I had completed my freshman year at New York University's West-chester campus. Soon after I had transferred to the University Of Michigan in Ann Arbor, I realized that chemistry was not my 'cup of tea'. I wanted to study something that was a bit adventurous and which offered opportunities to travel, go on expeditions. History, combined with many anthropology classes seemed to promise such a professional life style.

Q: After you had earned your undergraduate degree, you continued your studies at Columbia University. Why did you select to be involved with the Russian Institute, one of Columbia's programmes, and anthropology? Had

[1] The original interview with Professor Halpern was published in Serbian, as an Annexe to the first Serbian translation (revised and supplemented edition: 'Srpsko selo: Društvene i kulturne promene u seoskoj zajednici 1952–1987, *Etnološka biblioteka*, vol. 2, Belgrade: Srpski genealoški centar, 2006) of his book *A Serbian Village* (Columbia University Press 1956; second revised edition: Harper & Row Publishers, 1967). The purpose of the interview was to understand the circumstances under which a foreign scholar could conduct his field research at the time of limited freedom imposed by the communist regime. The point was to record the views of the researcher himself, to share the experience as seen with his own eyes and expressed in his own words. In a way it was a reverse situation of what Professor Halpern was doing in the field. While he was recording the villagers' way of life and their values, we were recording his views of the overall situation. In my view, it is particularly valuable information because it spans his experiences over an extended period of time, for almost half a century, so that it reveals the dynamics of change as well. Here he contextualizes his Balkan research within the total framework of his career.

you already made up your mind to pursue a university career, or were you perhaps interested in diplomacy?

I do not really know why I had chosen that particular path. My combined programmes at the Russian Institute with those offered at the Department of Anthropology very much appealed to me. I did not get any support from the anthropologists for this decision; it was my own unique academic path. I never got the formal certificate offered by the Russian Institute but I took a number of courses in Soviet Political Institutions, Marxian Economics, Russian Literature, Russian and Soviet History. I did, however, follow the anthropological curriculum leading to a doctoral degree which I received in 1956. The 1950s were at the height of the Cold War, and involvement with Soviet studies offered, in my mind, a specific, exciting, romantic thrill of the forbidden. Viewed from another perspective, from the very beginning of my graduate studies, I was interested in interdisciplinary approaches to the study of complex societies. Although my immediate objective was anthropological fieldwork, the possibility of a diplomatic career also crossed my mind. That is why, immediately after I defended my doctoral dissertation I took the formal government exams to become a diplomat working at the State Department. But, when an opportunity came later in the same year, I accepted a position to work in American foreign aid programme in Community Development in Laos.

Living in Laos, first as State Department employee from 1956 to 1958, and later exclusively as a researcher in 1959 and 1969, did influence my academic career. South-East Asia became the other important area of my anthropological interests. My first stay in Laos, which also happened to be the longest, also included a month-long visit to India where I became familiar with the community development programme. On my consequent Asian stays, I travelled to Thailand, Nepal and Vietnam. My work was not related to US military activities in the area, although, of course, all official US political activity was aimed at fighting communism. I, however, was glad to have the opportunity to serve as an advisor to the American government working with the United Nations Mekong Committee. This work involved dealing with the socio-economic consequences resulting from the building of huge hydro-electrical dams on the Mekong River.

Although there was no reason for me to hide anything, during the communist period in Eastern Europe, I was very careful to keep these two geographic areas of my interests, my work in South-East Asia and in the Balkans, separate. But this never was entirely true. Thus the Introduction to the first edition of *A Serbian Village* was written while I was resident in Luang Prabang, then the royal capital of Laos. The activities of the king and

his court in those days were wedged between the two Indo-China wars, the one that the French had lost in Vietnam and the subsequent war that the Americans were about to start in the following decade, ending with the fall of Saigon in 1975.

For a time in the late 1960s through the 1970s I thought that American policy towards Yugoslavia was much better conceived than our policy in South-East Asia, largely because we had less room for political manoeuvring and manipulation in the former. But in retrospect, the anti-Soviet communist motivation for support of Tito's regime seems more questionable as a policy momentarily convenient for both sides at the same time. The inability, or lack of desire or, more precisely put, the opposition of Titoism to any meaningful support for a democratic society based on some sort of the West European model was of little concern to the USA at that time. Consequences of the absence of this concern still haunt the present. The rural people such as those of Orašac were never given a chance to freely use their imagination and skills to promote the development of their country. Instead a phony social democracy, as exemplified in workers' self-management, was forced upon them. Also, as Djilas first began to observe publicly during our first visit, a new elite of Communist Party functionaries and Partisan war veterans had already composed a new privileged group. American imperial policies both in Yugoslavia and Laos seem to have been built more on opposition than affirmation. Retrospection is facile. The past in the future often lacks clarity.

Q: While in graduate school, you met Professor Phillip Mosley. As it turned out, it was a decisive moment for your career.

Phillip Mosley, a professor at Columbia and the first director of the Institute for Russian Studies was one of the founders of Eastern European studies in the USA. He taught history, international relations and specifically Soviet foreign policy. (For a time he was Director of Studies at the Council of Foreign Relations.) Something rather unusual had happened to him earlier. At the time he was applying for grants to continue his studies of Russian diplomacy concerning the Dardanelles, he was advised by the Social Science Research Council to go to the Balkans instead and study the societies there. He followed the advice, went to London School of Economics to study with Malinowski and then travelled throughout the Balkan Peninsula extensively. As is well known, he focused his research on *zadruga*, the extended family type, and published several important studies on the subject. Let me mention two more interesting facts about Mosley. First, he was one of the principal negotiators at the Trieste Peace Treaty. Second, he was a colleague of Mar-

garet Mead, and that is why she was chosen to write the Introduction to a collection of thematically very heterogeneous essays presented by his colleagues and followers from all fields of scientific research Mosley himself was pursuing at the posthumous conference in his honour held at the University of Indiana in the early 1970s. By the way, Margaret Mead was a member of my doctoral dissertation defence committee. For the American–Yugoslav/Serbian anthropological–ethnological relations, as well as for me personally, of crucial importance was the fact that during his research in Yugoslavia Phillip Mosley had established a close contact with Milenko S. Filipović.

Q: In addition to your academic advisor at Columbia, Professor Arensberg, and help you had from Professor Mosley in finding a suitable field-research location, you had acquired another, unofficial mentor on the other side of the Atlantic Ocean who also played a decisive role in your decision making process to come and work in a Serbian village. He continued to provide valuable advice and willingness to listen to your presentations and discuss your progress. Please tell us more about how you met, and then developed a professional association and friendship with Milenko Filipović.

His acquaintance with P. Mosley, helped Professor Filipović renew a grant from the Rockefeller Foundation for a year-long study abroad. Namely, in his original application Filipović stated that he wanted to go to London and study with Malinowski and visit English villages, which he would then compare to the Balkan rural settlements. Unfortunately, the grant was approved in 1939, and when war broke out, Filipović decided not to become a war refugee, but remain in his country with his family. After the war, with Mosley's help he came to the United States in 1951–2. He spent most of his sabbatical at Harvard working with Albert Lord and other professors there. He met with Mosley in the fall of 1952 and that is how I met him. Filipović was also interested in meeting foreign researchers who had studied in the Balkans, Gusti, for example, who was engaged in rural sociological studies in Romania. Among Filipović's desires was also to expand his research from ethnological and anthropological themes to sociological ones. He regretted the absence then of a Sociology Department at Belgrade University. His second regret was that outside of an international congress in Bulgaria, he had no opportunity to study abroad.

The possibility to go to Yugoslavia was to me much more attractive than the other alternative I had, namely to join a Harvard research team, led by the sociologist Alex Inkeles. I believe the project was supported by the Air Force and the objective was to interview displaced Soviet citizens in

America and in Germany. To my mind this was not a real fieldwork experience and Filipović's suggestion that I come to Serbia and Yugoslavia was much more attractive.

When I first came to Belgrade in the summer of 1953, I went straight to Filipović who was at that time working at the Ethnographic Institute of the Serbian Academy of Sciences and Art. Soon after, I met Professor Drobnjaković, the head of the Ethnological Department (School of Philosophy, University of Belgrade) at that time, who was also doing research in Šumadija with his two assistants, Srebrica Knežević and Milka Jovanović. At that time they were conducting research in Jarmenovci, a village in Šumadija, which had been selected as one of the locations for a United Nations rural developmental programme. It was in Jarmenovci that I first encountered Serbian folk culture and that was essentially the beginning of my research process. However, I soon moved on to Orašac, a village known as the site of the First Revolt against the Turks in 1804. Although both Jarmenovci and Orašac were to be sites of the UN's rural development programme, it was only in Jarmenovci that a small factory for fruit processing was eventually built. The United Nations had in mind a community development type of programme but the Communist government felt that they had no need of Western ideas about rural development, but they were very interested in the equipment and financing that the programme provided.

As a consequence, my initial visit to Orašac and my permit to do research was sponsored by the Central Council of Agricultural Cooperatives Federal Commission for Cultural Relations with Foreign Countries. During our first one-year long stay in Serbia, my wife, Dr. Barbara Kerewsky-Halpern and I visited Professor Filipović in his home on Sundays whenever we were in Belgrade. We were always invited to stay for dinner and enjoy a delicious meal prepared by the professor's wife and daughters. (One of them, Radmila Filipović-Fabijanić, was then just beginning, I think, to study ethnology at the University of Belgrade.) He patiently listened and critiqued my approaches to the fieldwork in Orašac. In addition to providing me with ethnological insights based on his long fieldwork and knowledge of Serbian ethnological literature, he also helped me become familiar with the classical writings in the field and with the broader cultural aspects of what was then Yugoslavia. Also thanks to his broad views I discovered valuable publications by physicians concerned with rural health. Milenko Filipović was an exceptional scholar in many ways. He was one of the best products of Cvijić's school, but he was at the same time very successful in tracing his own path, in carving an alternative approach focusing on dynamics and functions of the culture he simultaneously studied and belonged to himself. His Serbian colleagues were more locally oriented, with their vision turned inwards,

solely to their own folk culture without comparative insights or sociological interests.

Q: You and your wife were newlyweds when you first came to Serbia. Your relocation from New York City or its suburbia to Orašac must have been accompanied with something like a cultural shock?

I was in Europe for the first time in 1949 when I travelled through some parts of northern France, the Swiss Alps and northern Italy on a bike. I went as far as Naples, and from there I hitchhiked to Scandinavia. I spent some time in Sweden with the Saami (Lapps) just as they were rounding up their reindeer. The following summer I was in northern Alaska, on the Suard Peninsula, among the Eskimo. There, I worked with an archeological team, and I also made my own adventures, visiting a gold mine and joining local people hunting for small white whales (Beluga). Again, I hitchhiked back to New York. During the early years of my graduate studies I joined other expeditions, for example an archeological dig in New Mexico, and as an undergraduate I searched for fossils in Michigan's Upper Peninsula, and explored for uranium in northern Ontario.

Q: Still, all these were more summertime adventures and various professionally related skills-acquiring instances than experiences of extended fieldwork as defined by Malinowski. While it is certain that your familiarity with quite a few foreign countries and their cultures were helpful in your getting used to living in a Serbian village, it was still an entirely different predicament. Could you please describe your first impressions, apprehensions, hopes and desires once you landed in Serbia.

What you say is quite true and our time in Serbia, especially in Orašac, represented my first major fieldwork. All this happened a very long time ago. In 1956 Barbara wrote in a form of a memoir about her, our impressions of life in Serbia at that time. Unfortunately, it remains unpublished. So do my field notes which record our impressions and experiences in detail. But my dissertation which became the book, *A Serbian Village*, deals with life then, as it was more than fifty years ago. A lot of time has passed by. It was so long ago that I fear, if I were to return to Orašac now, I would find most of my past acquaintances, friends and informants buried in the village graveyard. If we place our first visit to Orašac in a broader time perspective, it took place more than one quarter of the time since the First Uprising and that event seems to have taken place so long ago (the 200th anniversary of the First Revolt was celebrated in 2004 and we were first there at the time of the

150th anniversary in 1954). Still, I clearly remember, that our first time in Orašac, in spite of scarcity, political repressions and isolation of a dictatorial regime looming like Damocles' sword over everyone's head, was very pleasant. It was before the time when piles of plastic waste grew everywhere as monuments of new time. In those days everything was re-used multiple times until much of the waste seemed to degrade naturally without a conscious effort. In those days there were no privately owned vehicles, and women on public transport would lift their skirts in a certain way before taking a seat so that the skirt would last longer. It was also the time when the differences between village and city dwellers, in patterns of speech and dress were readily discernible. A few of the older men still chanted the heroic epics accompanied by a single-string *gusle*, and they still had attentive listeners.

Beginning in the 1960s when the radios became almost universal and the TV made its appearance in the village, there was a competition of a football match or a sit-com. No one seemed to have much time for a recitation of an epic. But in the early 1950s, autumn evenings were spent around a fire, where the crops were harvested and women did their spinning while young people sang folk songs amidst thoughts of courtship. Relationships among people were direct and warm, without the destruction of mass media with its passive audience. On holidays, especially the *slave*, the celebration of a family's patron saint, involved the visit of a parish priest to individual homes. This took place despite the dominance of communist ideology and its atheistic orientation. The government was keen to replace religious celebrations with state holidays and everywhere in public spaces were the images of Tito. *Slava* was interesting also because it reaffirmed, through hospitality rituals, relations between affinals (groom's and bride's families, kinship through marriage) in otherwise emphatically patriarchal and patrilocal society. Other traditional festivities were also at variance with the official state holidays, along with regime-staged demonstrations and rallies.

Yes, my memories obviously have a romantic aura, but life was filled with hardships for all. Children were sent early from warm homes into the morning cold, wind and blizzard, to walk alone or with other pupils from the neighbourhood along back-roads and, if necessary, through snow drifts to the village school often some kilometres away. Lard might be rubbed on hands to ward off the cold when there were no mittens.

We felt privileged in Orašac and other villages that, despite political propaganda about the negatives of Western capitalist societies, we, two young Americans (they referred to us as *'Vi-Dečko'* ('you-boy') and *'Mlada'* ('bride')) were almost universally cordially welcomed into the villagers' homes. At that time they looked to America with respect, and even envy, as

a country whose standards and achievements should be emulated. They used to say: 'Give us fifty years without a war, and Serbia will become a small America.' Although World War II atrocities were not forgotten or even healed, and they had to live through daily hardships of trying to survive economically, the villagers were proud, dignified, secure in who they were and their hopes for future, confident in their own abilities. Optimism was radiating from almost every single villager. The following episode illustrates this point well. Our hosts were the proud owners of several pieces of high-quality German-soldier's stainless-steel mess kit which stood in sharp contrast to the poorly manufactured domestic aluminum cutlery which easily broke. The difference to them was a positive sign that even though at this time Yugoslav industry was still not able to produce adequate silverware, they were able to defeat the well-equipped German war machine.

I am not saying that there were not 'cultural surprises' when we encountered different customs. These surprises started as soon as our first trip to Orašac. We took a train from Belgrade to Mladenovac, and then a narrow gauge railway to Arandjelovac, the county seat. From there to Orašac we rode in a horse-drawn cart that belonged to a villager who was returning home and who was asked by the President of the Village Council to bring us along. Just as my wife was getting ready to climb into the seat next to him, he motioned to her to climb to the back of the cart, already filled with geese. I, as a male had the privilege of taking the front seat. My wife and I quickly became aware of the differences in defining and performing gender roles in a patriarchal setting.

All these were truly valuable experiences. On our later visits to Orašac we brought our children, our three daughters with us. They also share fond memories of the times spent in the village. Today they are all married women with children of the age they were at when they travelled to Orašac. The villagers were particularly fond of our oldest daughter, a physically able teenager, because she was strong enough to perform a number of farming tasks. We have a lot of photos of her working in the field, stacking hay and carrying out other chores. Our middle daughter also worked in the field, took care of the cattle, pigs and poultry and helped our hostess, Ljubica Stojanović with house chores. It does not come as a surprise that Orašac and Serbia have left a lasting mark on their lives.

A lot of time has passed indeed. *A Serbian Village* is now something of a historic document about a time and the lifestyle that then prevailed and has now, in certain essential ways, disappeared. My photographs, of which only a small number are printed in the Serbian village books, have a related significance. They are the visual evidence of the pre-industrial era and of a dynamic culture with a strong local flavour. Even now it is still possible to

recall the excitement of a Saturday market in Arandjelovac. It was an integral part of the village life, with visits to sell livestock or produce and, in turn, purchase items from town artisans and vendors from other regions, and take care of other business. Many artisans' shops began to disappear even by the 1960s. But some of the market activities persisted to the late twentieth century and were captured in the 1980s TV film 'The Halperns in Orašac'. There were other aspects of town market days and fairs. It was also a time for young villagers to get acquainted, as groups of marriageable-age girls and boys strolled along the main street, decked in their best attire, participating in the 'looking over' process. The drama of market day was part of the overall market experience. As for example, at the livestock market men would bargain and then proceed to the final stages of negotiating by slapping each other's hands until they shook hands on the price of the final trade.

Of course, the passage of time did not only witness the disappearance of the old, but also the introduction of the new and positive. We personally experienced the significance of a series of economic developments. When we first came to Orašac, the household we stayed at, although belonging to a rather well-to-do family, had neither electricity nor running water. They did not even have their own well. However, even towards the end of our first stay, electric power came to the hamlet, which was, of course, a much anticipated event. Schoolgirls from the neighbourhood created corn embellished chandeliers to adorn the new light bulbs. But even more significant was our hosts' digging of their own well in their own yard. Ljubica, our hostess, did not have to go down to a glen to a rather distant but helpful neighbour to fetch the water from his well. She no longer had to shoulder her yoke of heavy buckets after filling them with water and carry them up the quite steep hill several times a day. Barbara's job was to fetch water from distant ponds (about 800 metres away) that was given to the cattle. Having their own well really helped women with their traditional chore of supplying water for the household, farm animals and watering their vegetable and flower gardens.

Q: How did you decide where to live in Orašac, or was it decided for you by somebody else?

No, we did not choose ourselves. The decision had been made by the Village Council at a meeting held before we came to Orašac. The household of Žarko Stojanović, the Council Secretary at that time, was selected as our residence. It was a politically safe choice since Žarko was a state employee. In addition, his wife, Ljubica, was regarded as one of the best housekeepers in the village. Žarko's parents, grandpa Mileta and grandma Radojka, and

their children, Ruža and Milan, lived with them in the same household. We were very fortunate to enter into a three-generation household. On our later visits we continued to stay at their house and in time we have become really good friends, especially with the children. We followed their lives, shared their happy and sad moments.

It is a lasting pleasure for us to stay in touch with Milan and Ruža and their families. We were deeply touched when Milan called us at the time of the 9/11 incident to determine if we were all right and that no one in our family was harmed. I had been in touch with him in 1990s when he went through hard economic times like so many people in Serbia.

We were very fond of the entire Stojanović family and all considered ourselves to be close friends and we think of their hamlet as our second home. We were happy to witness any economic development and in the early1980s we were happy to contribute to the voluntary fund to build a road in their part of the village. This marked the end of a difficult dirt road on which it was equally hard to walk and drive. The soil had a clay-like consistency which was a good raw material used by the electric insulator factory in Arandjelovac, but very slippery when wet.

Q: You mention repressions against the villagers?

Having contacts with Americans at the time of our first stay in the village was not without risk. I remember one event that took place soon after our arrival in the village. Our first stop was the village coffee house (kafana). We were soon approached by a woman whose brother lived in Chicago. She asked us why we had come from America to this 'God-forsaken place'. She disappeared after that and we did not see her for several months. She was in jail. Once by chance I witnessed an instance of physical repression. I went to the police station in Arandjelovac to take care of some visa issue. I happened to open a wrong door and I saw policemen beating up a villager. I myself was a victim of organized professional demonstrators, official thugs, at the time of the Trieste crisis. They physically attacked me in the entry hall of the apartment building in the very Belgrade centre where my wife and I rented a room. I thought that it was a mistake, that they had confused me with someone else, for I had read in the Belgrade paper that foreigners were not to be harmed in the demonstrations. They only slightly bloodied me, and they did not use bars or heavy implements; there certainly was no attempt to kill me. They left me after I began yelling in English, which convinced them that I was indeed a foreigner.

All later trips to Serbia, during the 1960s and 1970s, until 1990 when my wife and I were there last together, were connected with unpleasant

hardships of acquiring permits for field research and living under the suspicious scrutiny of the local authorities. Clearly, a constant attempt was made to keep us on a short official string. During our first long stay we were granted visa permits for only a few months at a time. Then on each occasion as the permit was about to expire we had to return to Belgrade to renew our permit. It was not a very pleasant task to return to the major internal security headquarters of what was then called the UDBA (severely damaged by the NATO bombs in 1999) on Knez Miloša Street, just a few blocks down from the American Embassy. It was really tedious to repeat the same procedure over and over again, and I admit I was burdened with severe self-censorship. I was always very careful not to make a wrong move, not to say a wrong word, or write a wrong sentence, to avoid anything that could jeopardize our chances to go back to Serbia and Orašac. Although from today's perspective when inquiring journalists can be beheaded in some countries our travails may seem minor, and they were, the scrutiny and obligations nevertheless created something of a garrison state of mind when dealing with this sort of officialdom.

But changes did take place from the time of the very restrictive 1950s. For example research topics like the Social Policy Programme of the Institute of Social Studies in the Hague, Netherlands, such as, Socialism and Democracy conducted in two Šumadija villages in 1971 and 1972 (the Report, consisting of five parts, was published in August 1972), was exactly the type of work I avoided in the 1950s and 1960s for fear of being too political and being banned from the area and possibly hurting the local people. Still, I cannot say that we lived in fear. The Yugoslav government at that time depended on American economic and military aid, and the worst thing that could have happened to us was deportation. However, we never really thought that that was even a remote possibility.

I will not forget the many ways in which the new ideological order inhibited us in our research. I was even more concerned to protect the villagers from any potential inconvenience or harm. I remember with particular displeasure a little essay contest I conducted in the Orašac school, in which I asked the pupils to write about the history of their village, about their early childhood, their favourite holidays and their aspirations in life. Small prizes were offered for the best essays. Fortunately, for whatever reason, the teacher did not read the essays. When I returned to Belgrade I read them very carefully and discovered that some of the children wrote in a politically incorrect way. One child, in particular, described how his section of the village, suspected of supporting Chetniks during World War II, had been burned down by the Partisan forces. I immediately became alarmed about the possible consequences to this child, and especially his family, if this

paper should fall into official hands. In the early 1950s World War II was the dominant time marker in all conversations ('before' and 'after The War'), but officially it was celebrated as the Partisan victory over the Germans and the internal enemies, the Chetniks. Every village had its monument to the Partisan dead, while the deaths of the Chetniks and others officially passed unnoticed. Thus, anybody exposing Partisans in a negative light would be thus questioning the authority of the State and find oneself in potential trouble. I suppose it would have been possible to remove the child's name from the essay, but there was still his handwriting. The discovery of such a paper in my possession would obviously have also imperiled my stay in Orašac. In any case, although I hated doing it, I immediately burned the 'offending' essay in the bathroom of our Belgrade apartment, which we shared with a Montenegrin minor official and his family. I flushed the remaining ashes down the toilet.

I was always were careful not to accumulate my filed notes and other materials in my belongings in Belgrade or in the village. I made sure they were mailed promptly out of the country, to my address in the States, often in ways other than through the Yugoslav mail. I never wanted to take a chance of someone making lists of local names or interpreting data in a possibly harmful way.

Q: It seems that it was becoming harder and harder to get all the necessary permits as the time went by. This in turn seems to be in sharp contrast to the alleged gradual democratization of the society from the early 1970s on. I met you and Barbara in 1978 when the Department of Ethnology asked me to accompany you during your stay in the village, because of the changes enforced in the 1974 revision of the Constitution and specific laws and regulations that stemmed from it. According to them, foreign researchers were allowed to leave the metropolitan area (Belgrade in this case) only if accompanied of a domestic 'opposite number'. That is how I had become your companion, your 'guardian angel'. It could have been a very awkward situation, but luckily for all of us, and contrary to your experiences with the authorities, you always had only positive rapports with colleagues throughout Yugoslavia. You knew that you could not conduct your research without my participation, and I knew that you were very relaxed in my presence, becoming more and more convinced as the time went by that I had nothing but professional interests in mind. Let me confess: the president of the Council in Arandjelovc found a moment when you were engaged in a conversation with one of his aids, to tell me, in strict confidence, that they expected me to report anything I may find questionable or suspicious in your actions. Contrary to his expectations I did not assure him that I would do

that, but that I was convinced that you would never do anything to cause them to become suspicious, with or without my reports. He understood me very well and nobody ever approached me again with similar suggestions there. Months after our return from the village, I was asked to go to the Ministry of Interior for 'debriefing', but I told them flatly that there was nothing to 'debrief' me about. They did not pressure me at all, but they did offer some advice to the 'young scientist' (perceived also as 'naïve', I am sure) how field research is a very sensitive matter and that a lot of seemingly harmless information can be used to weaken the defence of a country. You, your wife and I and my family have met many times since then, in Yugoslavia, USA or in a third country while attending international anthropological conferences, but I was never approached again. I guess I was a disappointment as a potential counter-espionage agent.

I had the distinct impression that things were always changing. In our stay in Yugoslavia in 1961–2 there was a much freer atmosphere than there was a decade earlier in 1953–4. Although I am not saying that I did not have some difficulties in 1964 while working with a group of American graduate students in Bosnia, generally speaking I know that the situation became much tenser again in the 1970s. That had to be attributed to the change in political course and its legalization in the 1974 Constitution.

But, as for our colleagues, it was always a great pleasure to work and socialize with them. There were many scholars in our and the older generation with whom we have had good collegiate relations. Let me mention a few. Dr. Miloš Macura, Director of the Serbian Statistical Bureau in 1953–4, and of the Federal Statistical Bureau 1961 helped us to obtain detailed demographic data, with names, from all post World War II censuses not only for Orašac, but for all other parts of the former Yugoslavia in which I had conducted research. It must be said that over the decades of my work there I obtained for Orašac a total of some forty databases of varying degrees of length. I have to strongly emphasize that at that time the Yugoslavs/Serbs were much more open and generous in sharing data with scholars than the US rigidly controlled census data which could only be released with names some seventy years later. Other colleagues have helped with me in collecting archival data. It is very difficult to single out any one of them, or even list all of those who have made a major contribution to my research. However, I do have to mention the village council clerk who had worked with me in great detail (of course, I was happy to compensate his overtime after office hours) and provided me with vital records and other local data which were extremely important in enabling me to do subsequent analyses.

As for other forms of collaboration with local colleagues, of course they could have been more elaborate, intensive, in formal (for instance joint projects, student exchange) as well as informal ways, but there were never any conflicts. Some officials in Belgrade showed a great degree of understanding the nature of our work and helped us when needed. If our colleagues were assigned special tasks, like you were, the outcome depended on their own approach and attitude. You for one, thought nothing of it. On the other hand, if a person had a separate political agenda, then the events could have taken a different course.

Villagers too were not giving in to the pressure from the authorities. Once, during our 1953–4 stay, the police from Arandjelovac came to speak with our hosts while we were away, possibly in Belgrade (so, yes, there was monitoring, I guess). I do not know if I ever knew or possibly cannot remember who was actually at home that day, but a story that came out of the encounter is as follows. Baba Radojka (Mileta's wife and Žarko's mother) spoke to the policemen and said: 'What a shame to think that your mother brought you up to be a policeman!' I am not aware that Žarko and his family suffered any disability for housing us on our visits for all those years. After all, his home was chosen as our residence by the Village Council.

Many years later, in 1987, when Kamenko Katić, a Belgrade television documentary reporter, was filming a story about us and our work in the village, for his very successful series 'Mentioned in Passing', a policeman came up to his crew at the market in Arandjelovac with the intention of chasing them away for 'illegal action'. The crew was quite angry, yelled at him that he did not know the law and sent him away in no time.

On the other hand, I will mention one instance when the authorities asked me to do them a favour. Obviously, I was under the watchful eyes of Yugoslav diplomats in the USA as well. I remember a ludicrous incident from the 1960s when word went out through Foreign Ministry channels to drum up support for Tito's nomination for the Nobel Peace Prize. One of the diplomatic representatives hunted me down at a family wedding in New York City to plead with me to write to Oslo to the Nobel Peace Committee to nominate Tito for this honour. One can imagine how desperate they were for support and what kind of pressure these diplomats were to engage in these derisible efforts.

Q: In spite of the precautions you so diligently took, you still could not avoid being connected to some alleged spying activities.

I was never officially accused of espionage but my name was published in Belgrade newspapers as having a connection to other foreigners' alleged

spying activities. Attention was directed mainly to Americans and American universities. These accounts first appeared in the late 1960s. For some time I had wondered how my name first came to the attention of the security police. In the early 1970s a student drew my attention to a book written by a retired US army general on psychological warfare, *A Serbian Village* was quoted as an excellent source for understanding Serbia and Yugoslav culture in general. Of course, I did not write a book for the needs of the US army or for any type of warfare. The general in question had come across the book in his independent research and he made the evaluation in compiling his bibliography. I deserve neither praise nor blame for the general's discovery. It should be stated, however, that much research in the USA on Eastern Europe during the Cold War period was financed by US government agencies and a significant amount was written about this. But, at the same time, it should also be stressed that there was a great deal of cooperative research by Yugoslav scholars, such as sociologists with colleagues in Western Europe and the USA. Some of it concerned applied interests as in the field of public health. There had also been a tradition of such cooperative research in pre World War II Yugoslavia, partly financed by the League of the Nations and also by organizations such as the Rockefeller Foundation. What was new in all of this and part of my experience was the hostility of the conservative security service of the Yugoslav state towards the West.

I mentioned earlier that there were some difficulties in Bosnia in 1962 and 1964. In 1962 I undertook research with groups of students and individual faculties in sociology, political science, law and other social science students, including ethnology, in various parts of Yugoslavia to gather information on socio-economic life, family structures, division of labour and other similar topics. This was based on a questionnaire that I developed with Vojin Matić, a professor of sociology and a specialist in methodology at the University of Belgrade, over a period of one year. When the field research had been completed I deposited copies of all gathered materials with all the pertinent university departments and research institutes that had, through their students' and associates' participation, taken part in this project. A part of the research conducted in Serbia was published in one of the publications of the Ethnographic Institute of the Serbian Academy of Sciences and Art. It was only in Bosnia that I did not deposit the materials myself. One of my research assistants, a nice, hard working law student, promised that he would deposit them for me. In the meantime he decided that he wanted to pursue a career with the police. Thus instead of taking the questionnaires, field notes, and other documents to be archived at the Faculty of Political Science at the University of Sarajevo, as he had promised, he turned them over to the police to help advance his career goals. As a result there was a lot of nega-

tive press in Sarajevo, but of course, no connection or proof of 'spying intent' could be found.

A little later, in 1968, another team of American scholars, William Lockwood and his wife Yvonne, working in the Bugojno area (at that time a part of Bosnia and Hercegovina, one of the former republics comprising the Yugoslav Federation), encountered their share of trouble. Only Lockwood, a student of the 1960s and its anti-government ideology, did not think of going to the American Consulate in Sarajevo to seek help through official channels. Instead they were deported and denied a re-entry visa. They had to give up their field research in Yugoslavia and continue to work with the Croatian minority in Austria.[2]

The second incident that involved me happened only two years later. In 1964 I was conducting fieldwork with a group of graduate students from Brandeis University in the environs of Vareš and Maglaj. The purpose of this summer expedition was not to gather any particular data but rather to provide American students with the opportunity to master some of the fieldwork techniques. There were no problems during our research stay in Maglaj. However, later on, in 1968, one of the students, Leon Bresloff, decided to come back to Yugoslavia, to Orašac and Bugojno in Bosnia, and continue the research on his own. Unfortunately he picked Bugojno, a sensitive area because of the presence of military facilities and industry. He then proceeded to publicly criticize the Yugoslav version of socialism and advocate the Albanian model of communism. In the spirit of the time, he was deported from the country as a spy, and *Oslobodjenj*, a major newspaper in Sarajevo, featured an article 'on the well known spies, Lockwood and Bresloff, pretending to do scholarly research'. My name was mentioned as well, not as a spy, but as a spy's professor. Ten years later a Belgrade daily, *Politika*, published an article and a separate brochure entitled 'Science and Espionage' in which a large number of American research projects were qualified

[2] William and Yvonne Lockwood were investigating the impact of markets and fairs on peasant life. However, the authorities claimed, which was all duly and loudly reported in Bosnian press, that there were multiple denunciations filed by ordinary civilians about Lockwood's transgressions of the scope of the research as it was described in his application. He had taken advantage of the villagers' hospitality and trust and ventured into areas of questioning that the informants did not feel comfortable with. He would, according to the witnesses, insist on male informants telling him where exactly they had served their army term. This was characterized as 'extracting information of military importance'. Further, he demanded that the villagers sing songs disparaging other ethnic groups and nationalities. If the informants refused and said that they did not know any songs with such content, he would play back the recordings he had previously made. This line of questioning was characterized as provoking 'inter-ethnic intolerance in Bosnia and Hercegovina'. The police believed that scientific research was only a smoke screen for espionage (*Oslobodjenje*, Sarajevo, 29 February 1968).

as works with a dual purpose: scholarship and intelligence. My name was listed there as well. It was only a newspaper article, not an official accusation, but still it was a very unpleasant experience. I was never denied a visa or a research permit. To the contrary, I protested to the Yugoslav Embassy in Washington for every mention of my name that implied espionage. My letter of protest was followed by an oral confirmation from a senior Yugoslav diplomat that I was welcome to return to Yugoslavia and continue my research there.

Q: This is not the end the 'spy novel' in which you were allotted a prominent role. There is still another bizarre sequel in which you play a very different role.

Yes, that last episode was truly the most bizarre of them all. I still cannot understand how all those connections were made. It starts in late 1980s or early 1990s when the weekly *Duga* published a brief biography on Lawrence Durrell, a well-known British novelist, poet, dramatist and travel writer, and the author of the critically acclaimed *Alexandria Quartet*. (It is known that he was a British intelligence officer during World War II in Cairo and Alexandria.) Durrell was the Press Attaché in the British Embassy in Belgrade from 1948 to 1952. During his stay he collected materials for another book, *White Eagles Over Serbia*, published in 1957. *Duga* also reprinted excerpts from this espionage novel, which depicted the authentic political atmosphere in Belgrade at that time and British attempts to destabilize Tito's regime in which Durrell played a significant part. The article was illustrated with a strange collection of photographs. Some of them made perfect sense. There was one of Mr. Durrell, a popular flamboyant *bon vivant*, admired in Belgrade for his unconventional lifestyle. Then there were a few images by the internationally famous German-born fashion photographer, Helmut Newton, known for his erotic studies of nude women. The photos representing beautiful nude women in provocative poses, juxtaposed by fully dressed male figures attentively and appreciatively, mostly visually, inspecting their naked bodies with an aura of fetishism. This fitted very well into the story of espionage, eroticism and mysteriousness of the profession. The remaining three photographs were, however, a total puzzle. They were my photographs taken in Orašac in 1953–4, reprinted without my knowledge or permission, and in violation of all copyright laws, from *A Serbian Village*, or more likely, from a more accessible source *Pregled* (*Review*), a magazine published by the American Embassy. In 1987, the magazine published a version of an article Barbara and I had written on long term research in Orašac and the dynamics of change in the rural culture. The article contained a number of my photo-

graphs, three of which have reappeared in *Duga*. One of my photos showed an elderly couple in front of their dilapidated, modest home. The other represented twin sisters and their family holding beaded chandeliers which was to mark the fact that Orašac was soon then, in the fall of 1953, to get electricity. The third one, however, was the most bizarre and intriguing. It represents us as a young couple dressed in Ljubica's and Žarko's Šumadija-style folk costumes that they wore only on special occasions and kept as their burial attire. The circumstance under which this photograph was taken was also unique. One day during our early stay in the village, in 1953, one of our friends, an American history professor and his wife, came to Orašac to spend a weekend with us. They thought that it would be fun to put on the costumes and take photographs dressed as Serbian peasants. We took turns, first our friends, then us. We posed at the door of the old house, which was not torn down after the new one had been built, but turned into a pig sty. I used this photograph for the first time in 1987, in the TV documentary series 'Mentioned in Passing', which featured 'The Halperns in Orašac'. After that the same image appeared in *Pregled*, then in *Duga*, and finally on the cover of the first Serbian translation of *A Serbian Village*. A few years ago I attached it to a brief article on the use of photography in anthropology. I never suspected that a snapshot taken for fun out of boredom would experience such an interesting history. The bizarre twist is in the figure caption in the *Duga* article. It reads: 'Healthy peasant component: a bastion against communist dictatorship.' What a turn in symbolic meanings: from a spy to Serbian peasant, defender of the nation against communism. There are actually many layers of meanings hidden in this photograph and its connotations. In the *White Eagles over Serbia* Durrell's characters, intelligence agents, camouflage themselves as Serbian peasants by wearing folk attire: baggy trousers tacked into awkward riding boots, greasy fur hat, typical woollen coat. The transformation was complete. I believe that any further comments are redundant.

Chapter 20
Concluding Remarks

Vintilă Mihăilescu, Ilia Iliev, Slobodan Naumović

To write about 'anthropology' during communism in this part of Europe proved to be a challenge: not only because of the lack of a critical approach to the history of the discipline in general (especially in Romania and Bulgaria), but also because of the lack of a 'trial of communism' and thus of the possibility of a critical approach to the discipline in its recent past; not (only) because of academic reasons, but because such a writing implies (also) some clear *personal standpoints* in ongoing and still troubled and troubling *social debates*, crucial for these societies and concerning mainly their retrospective approach of communism. Thus, having to place 'socialist anthropology' in the *longue durée* of the discipline, one has to face the problem of the very nature of communism too. Wondering if 'anthropology' was a 'frozen' discipline during these times, if it was totally in discontinuity with the former practices of the discipline or not means also to make statements about communism as 'frozen' history. Looking at the way the discipline was practised during communism and wondering if it produced some interesting and acceptable ideas, data collections, debates, theoretical problems etc. or just political fakes means also to make some statements about social science in communism, about liberty and constraints, creativity and ideology, etc. This questioning is further on linked to the issue of the scientific heritage of this period: is there something to take over for the sake of the future of the discipline or not? This also means that some positive legacy of communism is accepted or, on the contrary, that anti-communist cleansing has to be categorical and overthrow everything rooted in this dark past. Comparing 'socialist anthropology' throughout time and space, one can point to similarities, or search rather for differences – which also means to stress the unifying power of communist ideology or to consider the existence of a certain diversity of socialisms. The same for the Soviet and/or Marxist influence, in the discipline or in society in general, that can be considered as an unavoidable common denominator, or as a much more 'polyphonic' context.

All these issues – and many others alike – are differently perceived and approached by different categories of scholars. Elder and younger, insiders and outsiders, scholars who managed to keep their jobs and careers throughout socialism as well as after, and scholars who failed or had to leave the country, all of them are also positioning themselves by the very stories and interpretations they give about the fate of the discipline during socialism. Beyond such personal or group trajectories, the different national belongings are also differently voiced, involving (more or less) different idiosyncrasies and styles of argumentation. In this respect, Serbian articles are the most passionate and polemic because of the new meanings former anthropological texts and statements have achieved in the recent context of the country. Some texts are thus mainly personal testimonies and some are regular oral histories; some point mainly to the constraints and misfortunes of the discipline and some others remember the success stories of some eminent scholars; for some scholars it was just a national communist follow up of the standard nation-building and peasant-centred mythology produced by ethnography and folklore before socialism, for others this 'socialist anthropology' is almost another discipline. But most of them try to nuance their statements beyond received clichés about science and ideology during socialism and all have something to advocate for. The editors make no exception.

As already mentioned in the introduction, we have chosen to give 'voice' to all these different members of the 'anthropological community', considering that this diversity is part of its 'culture' and has to be approached as such by a proper 'ethnography'. *Diversity* may thus be considered, to some extent, as the shared stake of most of the papers in this volume and thus as the central point these concluding remarks have to advocate for.

Naming 'Anthropology'

Probably the main question we have to answer at the end of this volume concerns the very status of the discipline(s) during socialism. In doing so, we should start with the very names 'anthropology' was given in these contexts, and their institutional background.

The distinction between *Folklore* and *Ethnography* is reproduced from pre-socialist times, follows *grosso modo* the spiritual–material culture split, but is reshaped institutionally from time to time and sometimes ends by meaning different things. Thus, for instance, the two are building a more or less happy family in Romania, most of the time under the same institutional roof, while getting to a public and spectacular divorce in Bulgaria (see Vassil Garnizov's chapter). *Ethnology* has a venerable tradition in Serbia, as a 'national science' but also, to some extent, in the sense of *Völkerkunde* (see

Mladena Prelić's chapter), with no equivalent in the other two countries. As for *anthropology* itself (as a cultural science, and not as 'physical anthropology', which is present in different degrees in the whole region), it is almost totally missing, with some meaningful exceptions (the case of Zagorka Golubović in Serbia, for instance, or much less so of Vasile Caramelea in Romania). We cannot speak thus of 'anthropology' in the considered countries but as a kind of generic, umbrella-concept, over-arching the different active branches of this academic field. Neither can we speak about a 'socialist anthropology', bearing common features in all the countries during socialist time. The variable geometry of this professional space should be thus approached in some more detail, case by case and step by step.

Continuity or Disjuncture

Complementary, 'anthropology' during socialism has to be placed in time, in the *longue durée* of the history of the discipline. In this respect, the crucial question that is raised across the whole volume concerns its continuity, the nature and degree of this continuity or of its brutal change. Roughly speaking, one can conclude that, after a period of ideological internationalism when all the social sciences had to define themselves *against* the national sciences of the past, anthropology took over the nation-building mission of the discipline and adapted it to the genuine – and sometimes contradictory – needs of national communism.

The shift to internationalism after the communist takeover had different consequences for the discipline in the three countries. In Romania, given an old anti-bolshevist tradition and the strong anti-soviet feelings, the repression of the 'bourgeois' and 'nationalist' scholars was also stronger during the Soviet handhold, most of the former folklorists and ethnographers being silenced for some decades or even imprisoned. Given its equally old Slavophile tradition and pro-Russian oriented ethnography, Bulgaria experienced a much softer passage, most of the pre-socialist scholars being able to continue their academic career during socialism too. In Serbia, given its federal context, the passage was much more debated and complex, having to bridge the different ideologies of federalism, nationalism, and Yugoslavism with that of internationalism, and combining former roots of the state with a new, simplified, Soviet model (see Mladena Prelić's chapter). The further external conflict with Moscow and internal reshaping of the 'republics' made things even more complex and dynamic.

The (communist) nation-building mission that was ascribed to anthropology after this first period was even more diverse, according to the different and shifting strategies the communist parties envisaged for their countries. The national(ist) reaction of these parties was different in the three

countries because of the different nature of relations Yugoslavia, Bulgaria and Romania had with Moscow, and because the different 'ethnic' problems each of them had to face at home. But, beyond these basic differences, all three countries were confronted with some similar objectives.

Starting at different moments and developing in different ways, the search for independence was one such main common feature. It requested not only the prizing of one's own socialist nation, but also complex strategies of re-rooting this distinct and independent nation in a proper *past*, both specific and valuable, and that could stand for the glorious genealogy of the present socialist people. Under the guidance of the party's activists, anthropology had to provide this 'proper' cultural past. And so it did, in different ways and degrees.

One strategy was to go as far back in the past as possible, overemphasizing an autochthonist approach of one's unique and ancient local roots: it was the case of Dacian and Thracian ancestors in Romania and Bulgaria, giving birth to a real 'Thraco-mania' in both countries. Many customs and folk texts were thus considered as having such origins – and proving them in return. On another side, ethnogenesis-oriented anthropology also (ab)used another strategy: just as the Greeks considered themselves to be European before Europe, Romanians were claiming to belong to the civilizing people by their Roman origins, and the Bulgarians by being the people who gave the Cyrillic alphabet and the Orthodox Christianity to the Slavs, and thus civilized them. In both cases, anthropology was but recycling old themes for the sake of new masters.

Yugoslavia was in a more complex situation in this respect too, having to face not only its passage to socialism, but also the one to federalism. In this context, it had also to find its way through the strong and permanent ambivalence of love and fear of the 'nation'. Anthropology had thus to stage a *national* (i.e. positive) past of the present socialist–federative state, providing legitimacy for *all* its 'nations' and avoiding any *nationalistic* (i.e. negative) approach that could harm the very existence of Yugoslavia (see Mladena Prelić's chapter). Nevertheless, some classical ethnographic works were taking the part of one or another 'nation', thus turning to 'nationalistic'. It was the case, for instance, of Spiro Kulišić's ethno-genetic discourse about Montenegro, which had long run political consequences (see Gordana Gorunović's chapter). Due to this more complex and debated context, theoretical–methodological changes in Serbian 'anthropology' during socialism seem to have been more dramatic, rather breaking off with pre-socialist canons of the discipline than continuing them in another form, as it was to a large extent the case in Romania and Bulgaria.

Beyond these main trends, it seems that in all three countries there were some influential people, who used to have contacts with their Western colleagues before the war and who were trying, not without success, to continue the work of the pre-war generation, and to investigate similar topics with the same tools, beyond and against the party's requests. They were an embodiment of continuity and became an alternative to the communist ethnography. However, anthropology in the West was changing meanwhile, while most of these personalities were still sticking to the classical canon. As time goes on, they were turning thus to a more and more conservative approach, ennobled only by its anticommunist fame. After the fall of communism, this group and their followers were – and still are – fighting for the legacy of the 'true anthropology' (see Vintilă Mihăilescu's chapter).

Studying Peoples under Socialism

We can now turn to another main question: *what* was anthropology actually studying during socialist times?

The editors of the preceding volume coined a convincing 'answer': *studying peoples in the people's democracies*. But, as already mentioned in the introduction, *who is the (real) People?* was the very lasting question. Accordingly, and in order to suggest an answer to the above question, we have to ask then *who was this (real) People during socialism* in the three countries?

First of all, we have to point to the fact that the detailed analyses of conceptual and theoretical practices gathered in this volume suggest that, beyond the official 'wooden language' (and sometimes even embedded in it), there was an academic language, with its own stakes and complex meanings. A lot of concepts were adopted and adapted, produced by, coined and reshaped according to more or less contextual aims and interests. Even more: it seems that the official discourse was not always and entirely just a 'wooden language', some changes in vocabulary and accents proving to be the expressions of important strategic changes and genuine 'world-views'. The common and ideology laden term of 'people', for instance, proved to be such a concept with many faces and complex stakes beyond its almost trivial and propagandistic appearance. The issue of *the* people, the 'true and real' one, was thus again on the front stage of anthropology: who was to be included, and who to be excluded – and thus who was to be studied in fact?

From the very beginning of the national ethnologies, there was a strong trend towards excluding some categories from the body of the people. In Serbia, Karadjic gave preference to the peasants versus urbanites – and this rural dominance of the people was true in the whole region. In Bulgaria, the nineteenth-century engineers of the nation claimed that the rich village

'big men', (*chorbadjii*) did not belong to the nation (Slavejkov). In Romania, on the contrary, the free and wealthier peasants (*răzeşi*) were considered the core representatives of the People (Russo *et al.*). The proletarian internationalism of the fifties was thus a total reversal for anthropology too. According to this new credo, the discipline had to focus on urban workers and to exclude at least the rural elite (*chorbadjii*, *chiaburi*, etc.), and to give up its national/ethnic frame of reference for an imposed international one (see Prelić, Eretescu, Benovska-Sabkova). But this reversal did not have a long life in any of the countries: against the Marxist–Leninist ideology, the peasants were back rather soon as well as a new kind of nationalism.

The ideological trend was, on the whole, one of growing homogenization, glancing at the ideal of classless communist society. As in the case of the great Romanian national contest 'Singing Romania' or other similar festivals, the 'whole people', without differences of class, nationality and religion, will be thus staged. Using 'people' in this all-embracing sense is just the old wine of unity-seeking ethnology in the new bottles of classless communism-building (see Vintilă Mihăilescu's chapter). From the class-defined proletarian people and its class enemies in the 1950s to the future classless people of communism insistently evoked in the 1980s there was a huge shift anthropology had thus to follow and cover in its mission of staging the socialist people.

Nevertheless, especially in Yugoslavia, 'ethnic identity' and diversity were also paid much attention, with shifting interests from the ethnically neutral historical materialism to the pursuit of particularistic interests and national programmes of certain ethnic groups and changing geometry of inclusion and exclusion (see Mladena Prelić's and Gordana Gorunović's chapters). The ideologically explicit trend to homogenization was thus doubled by the building of hidden hierarchies of first-, second- etc. rank citizens or even the exclusion of whole 'invisible' ethnic groups (Gypsies in Romania, Muslims in Bulgaria, etc.). Thus, from the federative context of Serbia to the mono-ethnic policy of Bulgaria, the *people* had a very different physiognomy, forcing anthropology to invent and/or use different concepts and approaches in order to describe it. Sometimes these differences are rather hidden in so far as the same concept may be used with distinct or even opposite meanings – such as, for instance, the Soviet inspired *ethnos* in Bulgaria and Serbia.

In our line of interest, what should be accorded some further attention is the category of peasants and their 'traditional' or 'popular' culture as a key hypostasis of the people. Equated, roughly speaking, with the 'real people' during the nation-building process and the beginnings of national ethnologies, this category could no longer be representative of the socialist people,

but neither could it be dismissed or totally neglected. Different solutions have been thus invented in order to incorporate 'traditional culture' in some kind of all embracing present day culture of the whole people. Nevertheless, peasantry and traditional life went on fascinating scholars and laymen alike, urging for a kind or another of rescue research: 'we are the last persons who can still collect traditional data in a comparatively systematic way. These data are lost quickly', a Bulgarian ethnographer claimed (see Anelia Kassabova's chapter). This feeling of urgency and national pride allowed – and sometimes even sustained – systematic field campaigns aiming to collect and preserve representative data in national archives and museums. Even an uncompromising critic such as Asen Balikci had to admit that most of these contributions were 'of a very high professional standard'. Sometimes, this prizing of the traditional culture went further, forcefully preserving it or even literally inventing it, as in the extreme case of the national festival 'Singing Romania'.

Some debates have raised questions about distinctions and relations between traditional, popular, and mass culture, about urban folklore, etc., but usually without trying to displace the old-fashioned 'traditional culture'. In other words, national ethnologies in all three countries managed to reproduce to some extent their original object of concern – the *Peasant* and his *traditional culture* – in spite of all its social and ideological transformations. Some main lines of continuity were thus provided for anthropological research during socialism, but with the risk of a kind of escapism, presented as a 'politically free' position: 'Our ethnographic research was not subordinated to any kind of political situation. It was purely cultural processes we investigated …', a Bulgarian ethnographer claims (see Anelia Kassabova's chapter). In fact, this commitment to the traditional object of research and the will to reproduce and preserve it despite social changes and political pressures is responsible for the more and more parochial character of a large part of the discipline, during and even after socialism.

Reinvented as it was, the *traditional people* thus survived socialism. And so did the scholars with mission to study it, as well as their status: whatever the changing geometry of the people, the anthropologist always remained 'one of ours', and was (self-) presented as belonging to the 'real people' and voicing it.

Whatever was meant by *people*, not everything was considered interesting about this people and not everything it was doing had to be studied. Both choices and omissions were extremely symptomatic in this respect, the repertoire of recommended subjects being probably the most telling aspect of anthropology during socialist times. Thus, for instance, religion and religious aspects were usually banished from the 'rational' legacy – tradi-

tional customs and beliefs being thus essentially falsified. Nevertheless, religious customs could be studied in different ways at least in order to control them and 'modernize' rural life. Ethnic legacies were also strictly controlled, but in different and sometimes opposite ways, from the limited ethnic diversity staged by federal Yugoslavia to the ethnic cleansing of the Bulgarian 'revival'. On the contrary, other categories of the national patrimony such as poetry, music, dances, handicrafts, some types of customs, etc. became the objects of large-scale research projects, collecting and/or staging in all the three countries.

Daily life, rural and urban alike, became an object of interest in Bulgaria under the concept of *bit*, while in Romania, after the initial proletcultist pathos, almost only ceremonial aspects of rural life were worth being studied and preserved. Social organization, kinship and almost everything one would consider elsewhere under the banner of social anthropology were almost completely neglected in both Romania and Bulgaria, but much less so in Yugoslavia. Economic issues were also more present in Serbian anthropology, as for instance the literature on Zadruga and the '*zadruga*-type production', which was meeting Sahlins' 'domestic mode of production' via Marx (see Gordana Gorunović's chapter). In general, the diversity of approaches and objectives was greater in Serbia – even if accompanied sometimes by what Ivana Spasić calls 'the art of mutual neglect'.

All these kinds of (selective) descriptions of the past and present peoples were aiming, in different ways and degrees, at the remodelling of present society. In this respect, describing the customs of the people – as in the case of the Bulgarian *bit* for instance – was also seen as a means to change, i.e. to modernize the people's way of life (see Ilia Iliev's chapter). In a much more ceremonial way, the Romanian huge national festival 'Singing Romania' had similar aims, trying to produce what in our days we would call models of 'good practice', i.e. proper ways to feel and behave for the future communist people as 'one soul and body'. Following the Soviet model, anthropologists were even given the task to invent new rituals (marriage, funeral, etc.) for the new *Volk*, but the political leaders had to give up in this case.

In doing so, anthropology during socialism adopted in fact the *Bildung* mission the discipline had from its very origin, and adapted it to the new, socialist, contexts and requests.

How can a National Science be Marxist?

Finally, having addressed the question of its object, we have to turn at the end to the means, asking *how* has anthropology approached this multi-faced *people* of its concern? The fact that it was under strong ideological control is

above question. But, as one can see through most of the papers in this volume, the policy of the socialist state was not as strong and monolithic as this, thus providing room for some choices, debates and alternative approaches. Further on, the discipline had enough creative resources to use these opportunities and turn them to more or less new and valuable concepts and insights.

Soviet ethnography offered, of course, a model to be followed. But, after the Stalinist period, even Bulgaria, who was the closest to this model, managed to give another shape to soviet inspired institutions and to turn upside down central concepts like *ethnos*, for instance. And, after all, Soviet ethnography was not itself homogeneous.

The same complex influence was true for Marxism. In Romania, for instance, lacking a real left tradition of thought, Marxism was officially proclaimed throughout socialism, but kept on being a peripheral line of thought, with small if any real influence on anthropology. In Yugoslavia, on the contrary, Marx had some important followers and Marxist anthropology developed in one rather important dimension of the discipline, similar to the creative way this happened in Western anthropology. In Bulgaria, Marxist influence was always present and important, but also allowing for some genuine local adaptations. More important, in this respect, were the national brands of historical materialism, of supposed Marxist–Leninist descent, and which were imposing the frame of historical thinking in general. In particular, historical materialism was coexisting very well with evolutionistic approaches in anthropology (for instance the 'evolutionary-Marxism' in Yugoslavia – see Gordana Gorunović's chapter), thus recommending this special school of thought, especially for the 'historical' discipline of ethnography. But not even in this case and not even in Serbia were Marx or Marxist anthropology of central concern.

Across many of the chapters of this volume, one can thus become aware of a strange paradox: Marx and the main holders of Marxism in these reputed Marxist countries were rather marginal references in the long run story of anthropology during socialism. How is it, for instance, that precisely the most aggressive (Kulisic) or sophisticated (Golubovich) wielders of Marxist ideology and jargon in Yugoslavia, seem to be also rather marginalized? In Bulgaria, the people who worked hardest on Marxist terminology, finally were not included in the pantheon presented by Milena Benovska-Sabkova in this volume, and never got the positions of power, controlled rather by integrated nationalists from pre- and post-war generations. In Romania, trained Marxist–Leninists like Gulian or Vlăduțiu published guiding books, but these never turned into real references of the discipline,

the authors being politically imposed but not academically followed (see Otilia Hedeşan's chapter).

The reason is, in a way, simple: especially with growing national communism, the Marxist rhetoric was alien – and even scary – to the nation, having to be kept under the strict control of the high priests of the socialist state. It seems that 'anthropology' in these countries, as 'national science', had a structural problem in dealing with demonstrably foreign intellectual influences, in general, and with Marxist (proletarian) universalism in particular. Consequently, the Marxist rhetoric was neither challenged, nor actually subjected to discussion, but also never accepted in fact (at least in Romania and Bulgaria), besides some lip service in the ritual prefaces of the books.

On the other hand, one has also to take into consideration the different reactions against this kind of influence and against ideological constraints in general. Thus, in Bulgaria and even more so in Romania, some young scholars got involved in semiotics and structuralism, for instance, the formalism of these approaches allowing them to avoid to a larger extent the party's academic control. This initially reactive movement produced in time some consistent contributions, but was not really accommodated on the national soils of anthropology for opposed but nevertheless similar reasons as Marxism. In Serbia, the Western trained scholars were comparatively more numerous, making up a real alternative to main trend socialist framed 'anthropology'.

The fortune of the Marxist influence might provide some useful lessons for the newly emerging modern Western-style anthropology in these countries. The new intellectual and research paradigms it brings are certainly alluring, as was the case with the new internationalist ideas in the post-war decades. They enjoy the institutional backing of various international academic and research organizations, but it could hardly match the institutional pressure on the old ethnology in the 1950s and 1960s – and the local intellectual traditions and actors have been demonstrating an impressive array of coping strategies and potential to resist to foreign influences. This capacity for resistance could not be attributed exclusively to institutional inertia or to a provincial propensity to old intellectual fashions. Besides the local elites, constantly changing and constantly national, there is a genuine demand for nation-building sciences in the societies at the periphery of Europe and the former Soviet empire, as well as a demand for the images of the people produced by them.

Appendix: Timelines

Romania

I. 1877–1947: Antecedents

1877 State independence of Romania.
1877/1893 Bogdan Petriceicu Haşdeu launches the first ethnological questionnaire, concerning the juridical practices of the Romanians (*Obiceiurile juridice ale poporului român*). In 1893 Nicolae Densuşianu launches his questionnaire, focusing on the historical traditions' (*Cestionariu despre tradiţiile istorice şi antichităţile ţerilor locuite de români*).
1905–1909 First main institutions appear: C. Diaconovici founds the History and Ethnography Museum (*Muzeul de Istorie şi Etnografie*) of the *Astra* Society in Sibiu (1905). Al. Tzigara-Samurcas founds the Museum of Ethnography, National Art, Decorative and Industrial Art (*Muzeul de Etnografie, Artă Naţională, Artă Decorativă şi Industrială*) in Bucharest, the modern-day Museum of the Romanian Peasant. Ovid Densusianu initiates what will become his philological school, and Simion Mehedinţi inaugurates a course of Anthropogeography and Ethnography within the Department of Geography at the University of Bucharest.
1906–1928 The Romanian Academy publishes 39 volumes of ethnography and folklore with the title *Of the Life of the Romanian People* (*Din viaţa poporului roman*).
1919 Great Romania is established.
1925 The first monographic campaign of Dimitrie Gusti's Sociological School. This activity will go on until shortly after the war.
1926 Romulus Vuia becomes the first titular of the Department of Ethnography at University of Cluj.
1930 The Folklore Archive of the Romanian Academy in Cluj is inaugurated under Ion Muşlea's coordination.
1928–1930 The first courses of Folklore at the University of Bucharest (I. A. Candrea: *Dialectology and Folklore*; N. Cartojan: *Old Romanian Literature and Folklore*; D. Caracostea: *Modern Romanian Literature and Folklore*).
1926–1940 The Bucharest School of (physical) Anthropology starts with the anatomist Francisc Reiner. In 1930, I. C. Botez will become the first titular of a Chair on Anthropology and Paleontology in Iasi. In 1933 a Society of Anthropology is organized in Cluj. Finally, in 1940, the still existing Institute of Anthropology opens in Bucharest under the directorship of Dr Reiner.
1927–1940 Three leading ethnographic museums are opened in Cluj (1927), Bucharest (1936) and Iasi (1940).

II. 1947–1959: Communist Reform of the Discipline

1947 Petru Groza's first communist government takes the power.
1949 The collectivization of agriculture is decided by the Central Committee of The Romanian Workers' Party (*Partidul Muncitoresc Român*). It will 'successfully' end in 1962.
1949 The Institute of Folklore (the present 'Constantin Brăiloiu' Institute of Ethnography and Folklore of the Romanian Academy) is founded, under the directorship of Harry Brauner, an old collaborator of Dimitrie Gusti and Constantin Brăiloiu. It also has a department in Cluj. The 'New folklore' becomes a central topic for the first decade of its activity.
1951 The Institute of Literary History and Folklore (*Institutul de Istorie Literară şi Folclor*) of the Romanian Academy is founded under the directorship of George Călinescu, a leading figure of literary history. The folk studies undertaken under this authority, will in fact switch to literary studies and will turn to the social context and diffusion of folk texts.
1952 The Institute of Literary History and Folklore launches the series of *Studii şi Cercetări de Istorie Literară şi Folclor (Studies and Researches in Literary History and Folklore*). It is the first professional publication in communist Romania to include folkloristic and ethnographical studies, and publishes monographic research on a series of oral literature themes.
1953 The Central House of Popular Creation (*Casa Centrală a Creației Populare*) is founded, reproducing the soviet model of the 'N. K. Krupskai' *House of Creation*. Its major aims are to promote an *artistic movement of amateurs* and to collect 'new folklore'.
1953 The Institute of Literary History and Folklore starts a massive collection of folk texts, trying to impose a model in this domain. It is worth noticing that the corpus of texts does not include any piece of 'new folklore'.
1954 Mihai Pop, an important member of Gusti's Sociological School, former collaborator of Roman Jakobson and Piotr Bogatyrev, becomes Scientific Director of the Institute of Folklore of the Romanian Academy.
1956 *Revista de folclor* (*Journal of Folklore*) is launched by the Institute of Folklore (changed to *The Review of Ethnography and Folklore – REF* – after 1963). Mihai Pop signs the editorial, presenting his long-run research strategy: (a) the corpus of the Romanian folklore; (b) the thematic inventories; (c) the bibliography of Romanian ethnography and folklore; (d) the ethnographic atlas.
1957 The Marxist philosopher C. I. Gulian publishes *Sensul vieții în folclorul românesc (The Sense of Life in Romanian Folklore*), dedicated to the 'class characteristics of folklore'. Gulian's book will become a truly canonic text for the folklore seminars during the communist time.

1957 Mihai Pop replaces Mitu Grosu, an old communist militant, at the Faculty of Philology at the University of Bucharest, and takes over the Folklore courses.
1959 Ileana Vrancea, a minor militant of the communist system, publishes in *Lupta de clasă* (*Class Struggle*) a long paper against 'mystical-reactionary theories on folklore'. Although Mihai Pop, Ion Muşlea and Adrian Fochi (all of them important academic and administrative figures of the discipline) are concerned there are no administrative consequences. It will be the last Stalinist-like attack against the discipline.

III. 1960–1971: A Decade of Opening Up

1963 Folklore begins to be approached through a semiotic lens in a series of articles published in *REF*.
1964 The birth of 'cultural and social anthropology'. Vasile Caramelea, a member of the last generation of Gusti's co-workers, manages to impose on a small department the puzzling title of 'socio-demographic and cultural anthropology' at the Center of (physical) Anthropology in Bucharest. In 1972, he starts a course in 'cultural anthropology' at the Faculty of Philosophy in Bucharest.
1964 A series of representative publications start. The first volume of *The History of the Romanian Literature* assigns a quarter of its pages to folklore. Important corpuses of folkloric texts and journals (on *Mioriţa*, the national patrimonial ballad, on *Romanian Ballads*, on *Prose*, on *Literary Folklore*) are published without ideological interferences. Some of them are – and will stay up to 1990 – *pentru uz intern* (for internal use only), i.e. without official distribution.
1965 Mihai Pop becomes the Director of The Institute of Folklore. In 1971, he will be elected co-president of The European Society of Folklore and Ethnology, with Jean Cuisenier and A. J.Greimas. He will retire in 1974.
21 August 1968 Ceauşescu opposes the invasion of Czechoslovakia and claims Romania's sovereignty and need for its own international politics, unsubordinated to USSR.
1968 At Mihai Pop's initiative, the University of Timişoara starts a series of international folklore seminars. A great number of ethnologists from Germany, France, Austria, Belgium, Italy and ex-Yugoslavia will attend the almost twenty occasions.
1968 The beginning of a series of translations from Claude Lévi–Strauss: *Tristes tropiques* (1968), *Le pensée sauvage* (1970), *Anthropologie structurale* (1978).

1969 Official visit of the president of the USA, Richard Nixon. This will open up the possibility for American anthropologists to start fieldwork in Romania.
1969 *The International Narrative Conference* takes place in Bucharest. On this occasion, lectures are given by E. M. Meletinski, Richard Dorson, Lauri Honko and others.

IV. 1971–1990: The Years of Rising National Communism

1971 After his return from an official visit to China and North Korea, Ceauşescu launches the *July Theses*, thus inaugurating a kind of small *Cultural Revolution*. Soon, the concept of 'multilaterally developed socialist society' (*societate socialistă multilateral dezvoltată*) will be coined in order to suggest that Romanian society has already entered a new, more advanced, stage in the building of communism.
1973 Ion Vlăduţiu, Director of the Ethnographical section within the Ethnographical and Folklore Institute, publishes a soviet oriented *Romanian Ethnography*, coldly received by the specialists.
April 1974 The amalgamation they were brought together of the Institute of Ethnographical and Folklore Institute, the Centre for Phonetics and Dialectal Research and the former Central House of Popular Creation. The emerging institute (ICED) will combine research and ideological responsibilities, also being involved, with effect from 1977, in the coordination of the national festival and contest *Cîntarea Romaniei* ('Singing Romania').
1974 Ovidiu Bîrlea publishes a reference book on *Istoria folcloristicii româneşti* (*The History of Romanian Folklore Studies*). Two years later, Mihai Pop and Pavel Ruxăndoiu publish a didactical reference volume on *Folclor literar românesc* (*Literary Romanian Folklore*).
1974 By State Decree 225 Romanians are forbidden to host foreigners. Fieldwork becomes problematic for western anthropologists.
1976 In line with the *July Theses*, the Congress of Political Education and Socialist Culture decides the launching of a nationwide festival and contest called *Cîntarea Romaniei* ('Singing Romania').

The Propaganda Section of the Party requests the reshaping of the traditional life-cycle rites of passage according to the new socialist spirit. Involving the Institute of Ethnological Researches and Dialectology, the project will fail, in fact, shortly after.
August 1977 Miners' strike in the Jiu Valley.
1978 A first book by Mircea Eliade (*Aspects of Myth*) is published in Romania.
*** Several small and *internal use only* publications continue publishing important texts collections and less 'orthodox' studies (e.g. *The Folklore*

Archive's Fascicles in Moldova, *The Annals of the Folklore Archive* in Cluj, the Facla editing house in Timisoara).
1982 Romanian Television starts the weekly programme *Tezaur folcloric* (Folkloric Thesaurus), setting the trend for a new genre: the folkloric TV show.
1985 Romulus Vulcănescu publishes *Romanian Mythology*. Scientifically misleading, the volume is an important contribution to the Romanian national-communist movement known as *protocronism* – the view according to which Romanian culture has anticipated a lot of European cultural achievements.

Bulgaria

I. 1878–1944: Antecedents

1878 Establishment of an independent Bulgarian state.
1888/1889 A course on Slavic Ethnography and Dialectology starts at the Faculty of History and Philology, Department of History, University of Sofia.
1889 Ivan Šišmanov publishes *The Importance and the Tasks of Our Ethnography*, arguing in favour of a comprehensive 'narodouka' (literarily 'study of the nation'), encompassing ethnography and folklore, and investigating the national traditions in a comparative Balkan and Slavic perspective. The text remains programmatic for Bulgarian ethnography and folklore till communist period.
1891/1892 The course on Slavic ethnography is transferred to the Department of Philology, Faculty of History and Philology. Until the 1960s the teaching on ethnography and folklore is offered primarily at the Department of Philology, later the Faculty of Philology. Minor courses at the Faculty of Geography.
1892 A National Museum is established in Sofia, with a section on Ethnography.
1906 A National Museum on Ethnography is established, the first director Dimitar Marinov. The staff of the museum usually has a background in philology or literature.

II. 1944–1956: Communist Reform of the Discipline

II.1. Reform of the Bulgarian Academy of Sciences

1947 June Nikola Petkov, the leader of the opposition in Bulgarian parliament, is arrested, and executed after a short trial.

1947 The Institute for Narodouka is established on 1 October, in the framework of a reformed post-Soviet model Bulgarian Academy of Sciences. The staff of the institute is recruited mostly from the Institute for a Dictionary of Bulgarian Language.
1947 In December, the liberal constitution of Bulgaria is replaced by a new one, called after the name of the communist leader Georgi Dimitrov.
1949 An Institute of Ethnography with Museum (EIM) is established, after a merger between the National Museum on Ethnography and the Institute for Narodouka. Its first director is Prof. Stoyan Romanski, who between 1922 and 1947 directed the Department of Slavic Languages and Ethnography at the faculty of Philology, University of Sofia.
1953 The publication of *Annual Collection of the Institute of Ethnography with Museum* begins. The institute is entrusted with the task of investigating the everyday life of the working class, collectivized peasants, youth volunteer workers, and pre-war communist resistance movement

III. 1956–1972

III.1. From philology to history

1956 April The communist leader Vălko Červenkov is replaced by Todor Živkov, following accusations of 'cult of personality'. Todor Živkov remained leader of the Communist Party till 1989.
1956/1957 A course on Ethnography is taught at the Faculty of History (a minor course on Slavic Ethnography for students specializing in Museum Studies).
1956/1958 Forceful collectivization of Bulgarian peasants.
1958 Prof. Hristo Gandev, a historian, becomes director of the Institute of Ethnography with Museum, enhancing the institutional link between history and ethnography.
1959 Petăr Dinekov's *Bălgarski folklor* is published.
1959 Within the Bulgarian Academy of Sciences, the Institute of Ethnography with a Museum is shifted from the Department of Language and Literature to the Department of Historical Sciences.
1965 A section on Ethnography is established at the Department of Archaeology and Auxiliary Historical Disciplines, Faculty of History. The teaching on ethnography is gradually shifted from the Faculty of Slavic Philology to the Faculty of History.
1968 Second edition of Mihail Arnaudov's *Očerci po bălgarskija folklor* is published (first edition 1934). Several pre-war authors, accused of nationalism in 1940s, were rehabilitated in the mid 1960s.

1972 Prof. Veselin Hadžinikolov, a historian, becomes director of the Institute of Ethnography with Museum, confirming the status of ethnography as sub-division of history.

III.2. Institutionalization of traditional culture

1964 'Etara', an open air Ethnographic and Museum Complex is opened on 7 October.
1965 The First National Fair of Folk Art in Koprivstitza takes place from 12–15 August, with 4,000 participants.
1967 The Union of Traditional Artisans of Arts and Crafts is established.
1969/1970 A course on Ethnography is reintroduced into the curriculum of the Faculty of Geography and Geology, department of Tourism.
1971 Second National Fair of Folk Art in Koprivstitza, with 2,000 participants.
1976 Third National Fair of Folk Art in Koprivstitza, with 3,000 participants, 90,000 organized visitors.
1981 Fourth National Fair of Folk Art in Koprivstitza, with 12,000 participants.
1986 Fifth National Fair of Folk Art in Koprivstitza, with 18,000 participants, 2,500 researchers and specialists in folklore and ethnography, and over 100,000 organized visitors.

IV. 1972–1989: Ethnography versus Folklore

1971 Xth Congress of Bulgarian Communist Party, discussing individual stimuli for increasing the personal motivation of workers and peasants, in order to improve the productivity of their work.
1972 December Plenum of the Central Committee of Bulgarian Communist Party, issuing a complex 'Programme for improving the welfare of the population'.
1972/1973 An Institute of Folklore is established (16 October 1972, a decision of the Council of Ministers regarding the above programme; 15 March 1973, a decision of the Presidium of the Bulgarian Academy of Sciences). The section on Folklore of the Institute of Ethnography is transferred to the Institute of Folklore. The newly recruited staff has backgrounds in literature, philology, and arts, and focus their research on popular creativity and individual folk artists.
1972 An Institute for Thracian Studies is established at the Bulgarian Academy of Sciences (investigating non-Slavic origins of the nation).
1973/1974 The section on Ethnography is moved from the Department of Archaeology and Auxiliary Historical Disciplines to the Department of

Archival Studies and Auxiliary Historical Disciplines (archaeology became an independent department, while ethnography remained classified as discipline auxiliary to history).
1974 Bulgarian edition of Hristo Vakarelski's *Etnografia na Balgaria* (Polish edition in 1965).
1975 The publication of *Bulgarian Folklore*, a quarterly review begins. The *Annual Collection of the Institute of Ethnography with Museum* is transformed into another quarterly review, *Bulgarian Ethnography*. Both reviews are established following a recommendation of the Presidium of the Bulgarian Academy of Sciences, instructing each institute to have its own review.
1975 Todor Ivanov Živkov's *Folklore as Fiction* is published, launching a polemics between ethnogaphers and folklorists.
1980/1981 The Department of Ethnography is established at the Faculty of History.
1980–1985 Publication of the three-volume *Ethnography of Bălgaria*.
1981 1300 years of Bulgarian state is celebrated.
1983 Ivanička Georgieva's *Balgarska narodna mitologia*, a monumental study on traditional folk mythology is published.
1984 The process of forceful renaming of the ethnic Turks in Bulgaria is launched at the end of December.
1989 Stojan Genčev's fieldwork manual *Terennoto etnografsko izsledvane* is published.

Serbia

I. Late Eighteenth to Late Nineteenth Century: Founding Fathers

Late eighteenth century Serbian Enlightenment thinkers like Dositej Obradović discuss ethnological themes either in general treatises on the importance of science for national education, or in geographical writings.
1804–13 The first Serbian Insurrection against the Ottomans. The foundations of the future Serbian nation-state are laid down.
1818 Vuk Stefanović Karadžić (1787–1864), the great philologist-reformer and predecessor of Serbian folkloristics and ethnology, publishes his *Serbian Dictionary* (*Srpski rječnik*) in Leipzig. Numerous entries represent detailed descriptions of cultural traits of the Serbian peasantry. Karadžić's interest in peasant customs and beliefs signals the reorientation from Enlightenment towards romanticism in what was to become Serbian ethnology.
1867 Valtazar Bogišić, the great legal historian and jurist, publishes his *Guidelines for the description of legal customs…* (*Naputak za opisivanje*

pravnih običaja…). His pioneering scholarly and applied work will trace the development of legal ethnology in the region.
1878 The Treaty of Berlin recognizes Serbia as a fully independent state.

II. 1880–1940: Institutionalization and Professionalization of Ethnology as a 'National Science'

1880 The process of institutionalization and professionalization of ethnology commences. The course on comparative geography and ethnography is included in the curriculum of the historical-geographical section of the Great School. The inaugural lecture of the course is held on 3 January 1885.
1882 Milan Obrenović is crowned as the first King of Serbia.
1886 The Serbian Royal Academy of Sciences is founded in Belgrade as heir to the Serbian Learned Society and the highest-ranking institution for the coordination of scientific research in the country.
1888 The Constitution of 21 December introduces the modern parliamentary system into Serbia.
1894 The Serbian Royal Academy begins issuing *The Serbian Ethnographic Miscellany* (*Srpski etnografski zbornik*).
1898 The Ethnographic Board of the Serbian Royal Academy is founded.
1901 The Ethnographic Museum in Belgrade is opened. The Munich-trained naturalist and physical anthropologist Sima Trojanović becomes the first director. Ethnographic research and field collection of objects start in 1902.
1904 The first permanent exhibition of the Ethnographic Museum is opened on 20 September.
1906 First Ph.D. dissertations in Ethnology defended by the new generation of fully professionalized experts – Jovan Erdeljanović (Ph.D. dissertation on Montenegrin tribes) and Tihomir Đorđević (Ph.D. dissertation on Roma/Gypsies in Serbia).
1906 The Chair of Ethnology at the Faculty of Philosophy of the University of Belgrade is established. Jovan Erdeljanović and Tihomir Đorđević are nominated as first assistant professors of ethnology.
1912–13 The First and Second Balkan Wars.
1914–18 World War I. Many talented scholars perish in the successive wars.
1918 Jovan Cvijić publishes his monumental *La Péninsule Balkanique*. Cvijić offers the first comprehensive local scholarly 'template' – anthropo-geography. The template incorporates a consistent theoretical grounding, an elaborate research methodology, the regional monograph as a standard for the presentation of research results, as well as a clear conception of the social and political roles of the social sciences.

1919–20 As Head of the Territorial Section of the State Delegation at the Paris Peace Conference, Cvijić supervises the drawing of ethno-geographic charts which will delineate the frontiers of the new state – the Kingdom of Serbs, Croats and Slovenes.
1922 The processes of institutionalization and professionalization of ethnology in Serbia are finalized with the transformation of the Chair of Ethnology into the Department of Ethnology of the Faculty of Philosophy in Belgrade.
1926 The Ethnographic Museum in Belgrade starts publishing the *Bulletin of the Ethnographic Museum* (*Glasnik Etnografskog muzeja*).
1929 On 3 October 1929, the Kingdom of the Serbs, Croats and Slovenes is renamed as the Kingdom of Yugoslavia. The policy of a single Yugoslav nation in a centralized Yugoslav state is adopted by the King, Alexander I Karađorđević, which leads to his assassination in 1934.
1939 Jovan Erdeljanović publishes the first university textbook on Ethnology in Serbian (*Osnove etnologije*).

III. 1945–9: Together with Comrade Stalin

1945 In April the communist controlled Court of Honour of the University of Belgrade expels Veselin Čajkanović, member of the Serbian Royal Academy, prominent classicist, philologist and leading authority on the history of Serbian religion, from the University on false accusations. His personal library is confiscated. He falls ill and dies in less than a year.
1947 The Ethnographic Institute (Etnografski institut SANU) is created in the frame of the refashioned Serbian Academy of Sciences.
1948 In June the Resolution of the Informbureau signals the breakdown of the relationship between the Soviet Union and its satellites and Yugoslav communists led by Tito. Tito's regime sets out on an 'independent path to socialism' and punishes severely its renegades.

IV. 1950–74: On the road to socialism, the Yugoslav way

1950 The system of *self-management* is inaugurated as the new doctrine of internal political organization of Yugoslavia.

The Marxist philosopher and ethnologist Dušan Nedeljković commences a research project on Yugoslav partisan folklore in the Ethnographical Institute of the SASA. As a political *éminence grise* he will exert influence on the careers of colleagues.
1951/2 Milenko Filipović, the most talented ethnologist of the pre-war generation, receives a grant from the Rockefeller Foundation for a year-long study in the USA.

1952 The Communist Party of Yugoslavia changes its name to the League of Communists of Yugoslavia. Leading ideologues like the Slovene Edvard Kardelj experiment with the idea of the 'withering away of the state'.

The Ethnographic Institute publishes the first volume of its Miscellany (*Glasnik etnografskog institute SANU*).

1953 Špiro Kulišić launches his fervent ideological attack on the roles of ethnographic museums in the frame of pre-war Serbian ethnology defined as a national science (Idejna i teorijska osnova u razvitku naših etnografskih muzeja).

1953/4 Joel and Barbara Halpern begin fieldwork in the Serbian village Orašac.

1954–81 In a series of texts and polemical exchanges, the Marxist historian Branislav Đurđev offers criticism of ethnological solutions to the problem of formation of Montenegrin tribes, and proposes an 'ideologically correct' one, based on a Morgan–Engelsian reading of the role of gens in Vallachian *katuns* (pastoralist settlements).

1955 Kulišić broadens his attack on ideological foes to include the 'anti-historicist' functionalist school and its supposed local proponents like Milenko Filipović (*Antiistorizam funkcionalne škole u etnologiji*).

1963 Manojlo Gluščević, Filipović's talented student, sharply criticizes the conception of ethnology as a science specializing in the study of ethnos and ethnogenesis. The ethnological project is seen as based on 'hypothetical reconstructions' and 'putative history', and anthropology is presented as the way out (*Etnografija, etnologija i antropologija*).

1964 Špiro Kulišić and Mirko Barjaktarović offer a ferocious rebuttal of Gluščević's anthropological project, defending the conception of ethnology as a discipline that studies the emergence and development of ethnic communities.

In December, at the Eight Congress of the Communist League of Yugoslavia, Tito attacks the then official policy of Yugoslavism as an excuse for 'bureaucratic centralism, unitarism and hegemony.'

1965 Milenko Filipović publishes his groundbreaking study on the webs of reciprocity related to the institution of *slava* in Serbia (*Zvanica ili uzov...*). Because of his consistent problem-oriented, dynamic and comparativist approach, Filipović can be considered as the predecessor of the trend of anthropologization of ethnology in Serbia.

1967/73 The philosopher and sociologist Zagorka Golubović attempts to develop an integral 'Marxist and humanistic approach in anthropology'. This 'systematic conception' of a general science of man is based on Marx's concepts of *praxis* and *human generic being*, on the ideas of the Frankfurt School, and on American culture and personality theories.

1968 Dragoslav Antonijević, Nedeljković's *protégé*, proposes that Serbian folkloristics should engage in a fight against 'folklorism' and 'folkloristic kitsch', by 'accentuating the authenticity, originality, and purity' of 'autochthonous' folklore, and by putting folklore festivals 'firmly under the control of experts' (*Borba protiv folklorizma jedan od uslova stvaralačkom razvitku savremenog folklora*).

1970–1 Nikola Pavković and Đurđica Petrović join the Department of Ethnology. Both combine a strong interest in historical contextualization with a readiness to cross disciplinary boundaries. Their research in the fields of legal ethnology and material culture paves the way for a modernist, anthropologically oriented type of approach in Serbian ethnology, like the one demonstrated by Mirjana Prošić-Dvornić in her path breaking historical-anthropological study of the relationship between the transformation of dress-codes and urbanization and Europeanization of nineteenth-century Belgrade.

1973 Špiro Kulišić publishes his paradoxical lament on the fate of Marxism in Yugoslav social sciences (*Zajedno s marksizmom potiskivana je i napredna nauka*). In it, general trends of liberalization in Yugoslav society are interpreted as signs of an ideological conspiracy against 'progressive Marxist science.'

Zagorka Golubović publishes her *magnum opus, Man and His World in an Anthropological Perspective* (*Čovek i njegov svet u antropološkoj perspektivi*). The book is put on trial because of the chapter containing a well-argued critique of the Yugoslav socialist system.

The Ethnographic Institute of the SASA begins the study of effects of socialist industrialization on the villages in the region of the Đerdap Dam on the Danube (*Etnološka proučavanja Đerdapskih naselja*).

The Cultural-Educational Community of Serbia (Kulturno-prosvetna zajednica Srbije) in cooperation with local cultural-educational communities begins organizing the manifestation Competition of Villages of Serbia (*Takmičenje sela Srbije*), better known as Village Encounters (*Susreti sela*), which will remain the main official channel of 'socialist development of cultural life in rural communities' in Serbia for decades.

V. 1974–80: The Yugoslav Experiment Reaches its Internal Limits

1974 The new Constitution defines Yugoslavia as a loose federation, or as a *de facto* confederation. The system of national communism installed in each of the Yugoslav republics characterises the new political situation.

The Graduate Interdisciplinary Chair in Anthropology, headed by Zagorka Golubović, is established at the Faculty of Philosophy as an inter-

departmental body that brings together specialists in philosophy, social sciences, arts and humanities.

1976 The Ethnographic Institute of the SASA begins the projects on *Continuous Tracking of Changes in Popular Culture and the Transformation of Suburban Settlements* (*Stalno praćenje promena u narodnoj kulturi – preobražaj prigradskih naselja, 1976–80*). The study of social and cultural dynamics replaces the up to then dominant static approach of the regional monograph.

1977 Ivan Kovačević publishes his seminal polemical study on *The Influence of Romanticism on the Development of Our Ethnology* (*Uticaj romantizma na razvoj naše etnologije*). In it, Kovačević attacks the very 'creation myths' of Serbian ethnology, the charisma of its 'founding fathers', and the 'sacred words' of the discipline like 'custom' (običaj), 'people' (narod), or 'psychic types' (psihički tipovi). Kovačević thus becomes the champion of what will later be called the 'anthropological revolution in Serbian ethnology'.

1978 Kovačević continues his revolutionary project with a programmatic text on the *Semiological Approach to the Study of Rituals* (*Semiološki pristup proučavanju obreda*). Semiology, Structuralism and British reinventions of Van Gennep's *rites de passage* supply the main theoretical frameworks of the envisaged revolutionary project.

VI. 1980–1991: Political, Economic and National Crises

1980 On 4 May, Tito dies. A decade marked by steadily worsening crisis begins. It will terminate with Yugoslavia's bloody collapse.

Špiro Kulišić publishes his study on the *Ethnogenesis of Montenegrins* (*O etnogenezi Crnogoraca*). The once fervent Marxist anti-nationalist ends his scientific career as a promoter of key ideas of Montenegrin separatist nationalism.

1981 Ivan Kovačević defends his Ph.D. thesis on *Ethnology in Serbian Enlightenment* (*Etnologija u srpskom prosvetiteljstvu*). By offering a detailed analysis of theoretical conceptions and empirical findings of Serbian thinkers of the Enlightenment, he reinvents an adequate past for his project of anthropologization of Serbian ethnology.

1983 Pavković, Bandić and Kovačević, leading scholars of the second and third post-war ethnological generations critically discuss the main trends of development of ethnology in Serbia, providing at the same time a manifesto for the rapidly spreading modernist approach (Težnje i pravci razvoja etnologije u SR Srbiji).

1987 The Centre for Ethnological and Anthropological Research is created as a research unit of the Department of Ethnology.

1990 The first multi-party parliamentary elections are held in Serbia in December. The reformed communists represented by the Socialist Party of Serbia (Socijalistička partija Srbije) win the elections on a platform of 'social stability, peace and unity of Yugoslavia'. The former Party apparatchik Slobodan Milošević is elected president of Serbia. Political transition in Serbia is blocked at its very beginning. It will commence only in 2000, with the ousting of Milošević's regime.

Mirjana Prošić-Dvornić publishes her seminal article on *Ethnos and Strategy* (*Etnos i strategija*), offering a dynamic conception of ethnicity and a penetrating insight into the logic of its political uses in Serbia.

The Department changes its name to the Department of Ethnology and Anthropology. The trend of anthropologization of ethnology receives formal recognition. Self-ethnography remains the dominant research orientation to the present day.

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